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JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

Art. I.—*Contributions to Meteorology, being results derived from an examination of the Observations of the United States Signal Service, and from other sources;* by Elias Loomis, Professor of Natural Philosophy in Yale College. Eighth paper. With plates I and II.

[Read before the National Academy of Sciences, New York, October 24, 1877.]

The origin and development of storms.

In order to determine the circumstances under which storms originate, and ultimately acquire their full intensity, I selected from the published volumes of the Signal Service observations (September, 1872, to May, 1874), all those cases in which the barometer fell below 29·25 inches at any station. It was found that the barometer on Mt. Washington was frequently very much lower than at the neighboring stations, Burlington and Portland, indicating some peculiarity of this station. All these cases were therefore set aside, and reserved for separate examination. During the entire period under discussion, the mean height of the barometer at Virginia City was nearly a third of an inch lower than at the neighboring stations, and these observations were therefore eliminated from my list. There remained one hundred and forty-eight cases in which the barometer fell below 29·25 inches at some one of the other stations. Sometimes at the same hour the barometer was below 29·25 inches at a considerable number of stations all included within the same low area. In such cases only one of the stations is included in the table, viz., the station at which the barometer was lowest. These one hundred and forty-eight cases correspond to forty-four different storms, and are shown in the following table, in which column 1st shows the number of the storm; column 2d shows the date at which the barometer was below 29·25 inches; column 3d shows the least height of the barometer observed at that hour; column 4th shows the name of the station at which the given height was observed; column 5th shows the total rain-fall (in inches) for the preceding eight hours, at all the stations included within the area of low barometer; column 6th shows the date of the storm's origin so far as can be determined from the observations; column 7th shows the locality where the storm originated. In order to reduce the dimensions of the table, the localities are denoted by symbols whose significance is explained on page 5. Column 8th shows the total rain-fall (in inches) for the eight hours preceding the date in column 6th, at all the stations included within a circle of six hundred miles in diameter, inclosing the locality named in column 7th; column 10th shows the direction of an area of high barometer on the east side of the locality where the storm originated; column 11th shows the distance of this high pressure in miles, and column 9th shows the height of the barometer at that point. Columns 12, 13 and 14 show similar particulars on the west side of the locality named in column 7th. Several additional columns which I had prepared, I am obliged to omit in order to reduce the table to the limits of the page of this Journal.

Barometer below 29·25 *inches.*

No.	Date.	Bar. low-est.	Station.	Rain in low.	Storm originated.		Rain-fall.	High on East side.			High on We	
					When.	Where.		Bar.	Direction	Dist.	Bar.	Directi
	1872.											
1	Nov. 7.2	29·12	Portland, Me.	6·00	Nov. 3	Wy.	0	30.25	S. 70° E.	850	30·18	N. 72°
	7.3	·13	Quebec	2·43								
	8.1	·07	Quebec	6·75								
	8.2	·19	Quebec	·25								
2	30.1	·10	Quebec	2·26	Nov. 28	n. Mi.	0	·38	S. 56 E.	600	·71	West
3	Dec. 24.1	·15	Portland, Or.	1·84	Dec. 23	n. W.		·70	East	1250		
4	27.1	·17	Halifax	·75	Dec. 23	G. M.	0	·35	East	680	·70	N. 12
	1873.											
5	Jan. 2.2	·23	Milwaukee	6·09	Jan. 1	N. M.	0	·42	N. 80 E.	1675	·15	N. 82
	2.3	·07	Milwaukee	5·62								
	3.1	·09	Marquette	8·12								
6	27.3	·17	Halifax	·74	Jan. 25	Col.	0	·36	East	1060	·17	West
7	29.2	·15	Fort Garry	0	Jan. 28	n. Mo.	0	·72	S. 54 E.	920	·18	West
8	Feb. 6.2	·20	Fort Garry	2·54	Feb. 5	n. Mo.	0	·30	S. 49 E.	1600	·41	S. 20
9	8.1	·15	Halifax	2·05	Feb. 6	Ala.	1·39	·29	N. 62 E.	650	·48	N. 79
	9.3	·20	Halifax	·20								
10	21.1	·19	Buffalo	7·56	Feb. 20	N. M.	·01	·34	N. 70 E.	1100	·59	N. 62
	21.2	·04	New London	6·89								
	21.3	·17	Portland, Me.	1·17								
	22.1	·22	Quebec	1·09								
	22.2	·14	Quebec	·52								
	22.3	·17	Quebec	·72								
	23.1	·18	Quebec	·92								
	23.2	·18	Quebec	·08								
	23.3	·23	Quebec	·40								
	24.1	·24	Quebec	·33								
11	Mar. 15.2	·13	Grand Haven	4·69	Mar. 14	Dak.	·02	·46	S. 78 E.	930	·36	West
	16.1	·16	Montreal	1·05								
	16.3	·23	Quebec	0								

Barometer below 29·25 *inches.*

Date.	Bar. lowest.	Station.	Rain in low.	Storm originated.		Rainfall.	High on East side.			High on West side.		
				When.	Where.		Bar.	Direction	Dist.	Bar.	Direction.	Dist.
1873.												
r. 21.1	29·19	Portland, Me.	4·63	Mar. 16	n. Mo.	0	30·44	S. 58° E.	820	30·14	S. 40° W.	900
21.2	·19	Portland, Me.	1·78									
26.3	·16	Portland, Me.	2·98	Mar. 23	n. T.	0	·41	N. 16 E.	960	·14	N. 74 W.	1200
29.2	29·01	Oswego	10·42	Mar. 26	n. Mo.	0	·37	S. 84 E.	650	·11	S. 41 W.	900
29.3	28·80	Burlington	5·73									
30.1	28·87	Portland, Me.	2·78									
30.2	29·20	Portland, Me.	1·40									
g. 24.2	29·03	Halifax	1·12	Aug. 23	A. O.	0				·36	N. 54 W.	1000
24.3	28·99	Halifax	·72									
t. 25.3	29·18	Yankton	1·70	Sept. 23	Dak.	0	·11	S. 83 E.	435	·10	West	1080
26.2	·20	Fort Garry	5·13									
6.2	·10	Punta Rassa	6·40	Oct. 5	Cub.	.01				·39	N. 33 W.	1600
6.3	·02	Punta Rassa	5·46									
17.2	·23	St. Paul	5·94	Oct. 16	n. D.	0	·31	S. 70 E.	680	·41	West	1200
17.3	·02	Duluth	3·14									
27.2	·22	Portland, Me.	8·41	Oct. 25	Col.	·04	·50	N. 74 E.	710	·47	N. 72 W.	920
w. 8.2	·14	Eastport	5·10	Nov. 4	n. W.	0	·42	East	1200			
8.3	29·12	Halifax	2·21									
9.1	28·96	Cape Rosier	1·56									
13.1	28·99	Cape Rosier	3·78	Nov. 10	Neb.	0	·24	N. 73 E.	440	·38	West	850
17.1	29·00	Wilmington	3·91	Nov. 13	n. Mo.	0	·35	S. 31 E.	1520	·12	S. 16 W.	440
17.2	28·82	Norfolk	7·23									
17.3	28·82	Cape May	5·32									
18.1	28·66	New London	8·23									
18.2	28·47	Eastport	9·12									
18.3	28·64	Chatham	2·16									
19.1	28·97	Cape Rosier	1·98									
24.3	29·09	Eastport	3·14	Nov. 21	N. M.	0	·39	S. 82 E.	1460	·14	N. 52 W.	1090
25.1	28·82	Halifax	4·31									
25.2	28·80	Sydney	2·79									
25.3	28·78	Sydney	0·66									
26.1	28·85	Sydney	0·78									
26.2	29·17	Sydney	0·07									
c. 3.3	29·08	Escanaba	13·99	Nov. 30	Mon.	0	·70	S. 83 E.	1650			
4.1	28·91	Marquette	3·67									
14.1	29·14	Sydney	0·95	Dec. 11	N. M.	0	·37	S. 82 E.	1500	·28	N. 51 W.	1090
28.1	28·93	Portland, Me.	2·52	Dec. 24	n. W.	·09	·51	East	1075			
28.2	28·91	Eastport	4·29									
28.3	28·98	Sydney	·49									
31.2	29·23	Fort Garry	·00	Dec. 29	n. W.	0	·63	S. 74 E.	1400			
2.2	·09	Fort Sully	·33	Dec. 31	n. Mo.	0	·64	S. 60 E.	1750			
2.3	·16	Fort Sully	·47									
3.1	·05	Omaha	·35									
3.2	·14	Marquette	1·32									
15.1	·15	Chatham	1·56	Jan. 13	Ark.	·10	·50	N. 76 E.	900	·87	N. 47 W.	720
15.3	·23	Portland, Or.	·43	Jan. 14	P. O.	·01	·96	East	1070			
16.1	·11	Portland, Or.	·78									
16.2	·06	Portland, Or.	·41									
16.3	·14	Fort Benton	·21									
17.1	·00	Fort Garry	·59									
4.1	·03	Halifax	·39	Jan. 31	N. M.	·03	·64	N. 55 E.	950	·20	N. 82 W.	900
4.2	·03	Sydney	·54									
10.3	29·06	Halifax	66	Feb. 8	G. M.	·04	·54	N. 20 E.	1480	·22	N. 70 W.	750
11.1	28·78	Sydney	·79									
11.2	28·84	Sydney	·44									
11.3	29·13	Sydney	·30									
11.2	·21	Fort Sully	·34	Feb. 9	P. O.	0	·54	S. 81 E.	1200			
11.3	·17	Fort Garry	·76									
12.1	·15	Leavenworth	·27									
12.2	·08	Dubuque	·96									
12.3	·13	Escanaba	2·20									

Barometer below 29·25 inches.

No.	Date.	Bar. low-est.	Station.	Rain in low.	Storm originated. When.	Storm originated. Where.	Rain-fall.	High on East side. Bar.	High on East side. Direction	High on East side. Dist.	High on W Bar.	High on W Direc
	1874.											
34	Feb. 16.3	29·09	Quebec	1·76	Feb. 12	Or.	0	30.53	East	1060		
	17.1	·07	Cape Rosier	·30								
	17.2	·12	Cape Rosier	·06								
35	Mar. 1.2	·23	Fort Garry	·02	Feb. 28	n. Mo.	0	·38	S. 72° E.	1800	30·25	S. 15°
	1.3	·05	Fort Garry	·09								
36	6.2	·23	Keokuk	4·45	Mar. 3	Mon.	0	·30	N. 70 E.	730	·21	S. 18
	6.3	·16	Dubuque	4·26								
	7.1	·17	Milwaukee	7·78								
	7.2	·24	Alpena	3·98								
	8.2	·22	Eastport	2·38								
	8.3	·19	Quebec	1·66								
	9.1	·24	Quebec	2·32								
	9.2	·13	Sydney	1·07								
	9.3	·07	Eastport	·92								
	10.1	·03	Eastport	1·41								
	10.2	·10	Eastport	·99								
	10.3	·12	Father Point	1·56								
	11.1	·16	Sydney	1·13								
	11.2	29·01	Sydney	1·12								
	11.3	28·90	Sydney	·31								
	12.1	28·87	Cape Rosier	·91								
	12.2	28·90	Cape Rosier	·14								
	12.3	28·85	Cape Rosier	·40								
	13.1	28·86	Cape Rosier	·56								
	13.2	29·00	Cape Rosier	·66								
	13.3	29·12	Cape Rosier	·09								
37	16.2	29·12	Fort Sully	6·85	Mar. 13	Mon.	·03	·41	East	740		
	19.2	29·19	Quebec	1·77								
	19.3	28·97	Father Point	1·23								
	20.1	29·15	Sydney	·21								
	20.2	·24	Sydney	·08								
38	April 12.2	·17	Fort Sully	·40	April 11	Ut.	0	.45	N. 54 E.	940		
	12.3	·24	Fort Sully	·32								
39	26.1	·13	Boston	6·34	April 23	G. M.	·07	·17	N. 15 E.	1200	·12	N. 7
	26.2	·23	Eastport	3·19								
	26.3	·14	Halifax	1·92								
	27.1	·14	Sydney	·71								
40	30.1	29·11	Father Point	3·00	April 26	Wy.	0	·24	N. 66 E.	770	·19	West
	30.2	28·97	Father Point	1·66								
	30.3	29·11	Quebec	·54								
	May 1.1	·05	Father Point	·46								
	1.2	·22	Father Point	·88								
41	April 30 2	·13	Fort Sully	·43	April 29	Dak.	0	·38	East	620		
	30.3	·20	Fort Sully	·01								
	May 1.2	·16	Fort Sully	·08								
	1.3	·23	Omaha	·11								
	2.1	·25	Omaha	1·56								
	2.2	·17	Leavenworth	2·62								
42	8.2	·05	Fort Sully	·51	May 7	Mon.	0	·23	S. 54 E.	1370		
	8.3	·12	Fort Sully	·04								
	9.1	·17	Fort Sully	·92								
	9.2	·01	Fort Sully	2·04								
	9.3	·15	Fort Sully	·92								
43	25.1	·24	Alpena	7·08	May 21	Dak.	0	·20	S. 80 E.	440		
	25.2	·17	Ottawa	8·78								
	25.3	·24	Ottawa	5·19								
44	26.2	·16	Fort Sully	·06	May 25	Mon.	0	·20	S. 36 E.	550		
	27.2	·16	Fort Sully	3·14								
	27.3	·24	Fort Sully	2·64								

Of these forty-four storms, thirty-two occurred during the five months from November to March, and only two occurred during the four months from June to September.

The third column of the following table shows the number of cases in which a storm originated in each of the different localities named in column 2d. The first column shows the letters by which the locality was designated in column 7th of the table on pages 2 to 4.

Symbol.	Where storm originated.	Cases.	Symbol.	Where storm originated.	Cases.
P. O.	Pacific Ocean, north of Washington Territory.	2	n. D.	North of Dakota.	1
			Dak.	Dakota.	4
n. W.	North of Wash'n Terr.	4	Neb.	Nebraska.	1
Or.	Oregon.	1	n. T.	Northern Texas.	1
Ut.	Utah.	1	n. Mi.	North of Michigan.	1
n. Mo.	North of Montana.	7	Ark.	Arkansas.	1
Mon.	Montana.	5	G. M.	Gulf of Mexico.	3
Wy.	Wyoming.	2	Ala.	Alabama.	1
Col.	Colorado.	2	Cub.	Near Cuba.	1
N. M.	New Mexico.	5	A. O.	Atlantic O., near lat 37°.	1

We see that, of the forty-four storms here recorded, two apparently came from the Pacific Ocean north of the United States boundary; four others came from nearly the same locality; one came from Oregon and one from Utah; that is, eight appear to have had their origin on the west side of the Rocky Mountains. Of the thirty-six remaining cases, seven appear to have originated north of Montana, five in Montana, two in Wyoming, two in Colorado, and five in New Mexico; that is, twenty-one appear to have originated upon or very near to the chain of the Rocky Mountains. Seven other cases appear to have originated west of the meridian of 95° from Greenwich, viz: one north of Dakota, four in Dakota, one in Nebraska and one in Northern Texas. Six other cases appear to have originated west of the meridian of 83° from Greenwich, viz: one north of Michigan, one in Arkansas, one in Alabama and three in the Gulf of Mexico. Besides these, one appeared to originate near Cuba, and one in the Atlantic Ocean not far from Cape Henry. We thus see that our great storms are not confined in their origin to any particular locality, but half of them originate upon or very near the chain of the Rocky Mountains. More than two-thirds of the whole number originate north of latitude 36°.

The first stage in the development of each of these storms was an area several hundred miles in diameter, over which the height of the barometer differed but little from thirty inches, with an area of high barometer both on the east and west sides, and at a distance of about 1,000 miles. In the few cases in which a high barometer is not reported on both sides of the origin, it is because the area of the observations was not sufficiently extended. The mean value of the high barometer on the east side was 30·42 inches, and the mean distance 1,033 miles; on the west side the values were 30·31 inches and 977 miles. If the area of the observations had been sufficiently extended towards the north, it is presumed there would sometimes have been found three areas of high barometer within a distance of 1,000 miles from the locality where the storm originated. On Hoffmeyer's storm charts we frequently find three areas of high barometer and occasionally four areas of high barometer surrounding an area of low barometer.

These areas of high barometer are regarded as one of the causes, and generally the most important cause of the storm which succeeds. Two such areas of high barometer create a tendency of the air towards an intermediate point, and the currents thus set in motion are deflected to the right by the earth's rotation, whence results a diminished pressure over the central area. This diminished pressure causes a still stronger inward flow of the air, which results in a still greater depression of the barometer. Since the air presses in on all sides towards this area of low barometer, the area tends to assume an oval form, which may become sensibly circular if the winds are very violent, and the centrifugal force resulting from this revolving motion causes a still further depression of the barometer. This partial vacuum would be soon filled, and the inward movement of the air would cease were it not for an upward motion by which the inflowing air escapes. The air in its upward motion, carrying with it a large amount of aqueous vapor, is cooled, and its vapor is condensed, producing rain. The heat which is liberated in the condensation of this vapor causes a further expansion of the air, and increases the force of the inward movement of the wind. Rain is then one of the circumstances which increases the force of a storm, and it invariably attends storms when they have attained to considerable violence, as is shown by the rain-fall in column 5th of the table on page 2. This table generally shows a large rainfall whenever the storm is so situated as to permit observations on its eastern side; but when the center of the storm passes eastward beyond our stations of observation, the observed rainfall rapidly diminishes. See Nos. 1, 9, 10, 11, 23, 34, 36 and 37.

I have shown in my 7th paper, pp. 14, 15, that an area of low barometer of considerable magnitude may be formed and continue for several days with very little rain; but in these cases the barometer was never observed to fall as low as 29·25 inches; and it will be noticed that some rain was invariably reported whenever the barometer fell below 29·4 inches, and generally there was some rain reported whenever the barometer fell below 29·5 inches. I have found no storm of great violence which was not accompanied by a considerable fall of rain. Rainfall is not, however, generally the cause of that first movement of the wind which results in a great barometric depression. This appears from column 8th of the table on page 2, which shows that over a circle of 600 miles in diameter surrounding the locality where the storm originated, in thirty-one cases no rain for the preceding eight hours was reported from any station, and in only one instance did the total rain-fall within this circle exceed one-tenth of an inch. It may be said that in the neighborhood of these localities the stations were few in number, and that rain-fall may have occurred at intermediate points from which we have no reports. But in at least a quarter of all the cases, this circle of 600 miles in diameter included as many as four or five stations, so that we seem fully justified in concluding that generally the inward movement of the air toward a central area begins before there is any considerable precipitation of vapor.

After an area of low barometer has been formed, it soon begins to change its position. This movement appears to be mainly determined by the same causes which control the general circulation of the atmosphere. Throughout the United States (with the exception of the extreme southern margin) the average annual progress of the wind is from west to east, and this movement is determined by causes which are general in their operation, and cannot be permanently changed by the influence of local storms. When an area of low barometer is formed, the wind sets in both from the east and west sides to restore the disturbed equilibrium. The partial rarefaction of the air causes an inward pressure of the wind on the east side which is balanced by an equal pressure of the wind from the west side arising from the same cause. But on the west side there is an additional pressure eastward resulting from that cause which determines the general circulation of the atmosphere, and which would exist even if there were no local disturbance. This pressure is not limited to the center of the storm, but extends to the entire mass of air which is disturbed by the storm, and generally extends much beyond these limits, and at all points is exerted nearly in parallel lines. Moreover, the disturbance of the atmosphere by storms is mainly confined to the lower half of the atmosphere, while the regular movement of the upper half is much less disturbed. The force of this upper current from the west, combined with that of the lower half of the atmosphere, pressing upon the west side of an area of low barometer tends to fill up the depression upon that side. On the east side of the storm, it is mainly the lower portion of the atmosphere which presses inward, while the upper portion is still moving from the west. Thus while under the influence of the earth's rotation upon the inflowing current, the barometer is continually falling on the east side of the storm, the pressure is being restored on the west side; that is, the center of least pressure advances in the direction of the general system of atmospheric circulation. This reasoning applies not merely to the easterly motion of storms in middle latitudes, but also to the westerly motion of storms within the tropics.

There is a third circumstance which appears to have an important influence upon the progress of storms in the middle latitudes. The upward motion of the air which always prevails within an area of low barometer takes place principally on the east side of the low center, as is indicated by the position of the rain-areas described in my 7th paper. By this upward motion, the air which presses in upon the east side of the low center is prevented from restoring the equilibrium of pressure upon that side, while there is less of this upward movement on the west side of the low center. Thus the low center is steadily transferred toward the east, or the storm travels eastward.

The areas of high barometer which uniformly mark the commencement of a storm, invariably attend it during its progress eastward. During the progress of the storms recorded in the table on page 2, the average value of the high barometer on the east side was 30·39 inches, and on the west side 30·32 inches, which numbers are almost identical with the values found for the commencement of the storms, showing that although the wind had been blowing outward from these areas of high barometer for several days, the magnitude of the high barometer had not been diminished. Hence we conclude that these areas of high barometer must be constantly recruited by air which flows toward them in the upper regions of the atmosphere, and this supply evidently comes from the areas of low barometer. In other words, during the progress of storms, the surface winds are moving from areas of high barometer toward areas of low barometer; and the upper winds are moving from areas of low barometer toward areas of high barometer.

An inspection of column 4th of the table on page 2, shows that these cases of low barometer occur most frequently in the neighborhood of the Atlantic Ocean. This will appear from the following table, in which the stations are divided into three classes; one class including the stations near the Atlantic coast or the Gulf of St. Lawrence, a second class including the stations between the preceding class and the meridian of 92° from Greenwich, the third class including stations west of the meridian of 92°.

Near the Atlantic coast.			From the coast to long. 92°.			West of longitude 92°.		
Station.	**Lat.**	**Cases.**	**Station.**	**Lat.**	**Cases.**	**Station.**	**Lat.**	**Cases.**
Cape Rosier	48°52′	11	Marquette	46°33′	3	Fort Garry	49°51′	8
Father Point	48 31	6	Escanaba	45 46	2	Fort Benton	47 52	1
Chatham	47 3	2	Montreal	45 31	1	Duluth	46 48	1
Quebec	46 48	17	Ottawa	45 26	2	Portland, Or.	45 30	4
Sydney	46 8	17	Alpena	45 5	2	St. Paul	44 53	1
Eastport	44 55	9	Grand Haven	43 5	1	Fort Sully	44 39	17
Halifax	44 39	11	Milwaukee	43 3	3	Yankton	42 45	1
Burlington	44 29	1	Buffalo	42 53	1	Omaha	41 16	3
Portland, Me.	43 40	9	Dubuque	42 30	2	Leavenworth	39 19	2
Boston	42 21	1	Oswego	42 28	1			
New London	41 22	1	Keokuk	40 23	1			
Cape May	38 56	1						
Norfolk	36 51	1						
Wilmington	34 11	1						
Punta Rassa	26 29	2						

We see that cases of low barometer occur most frequently at the northern stations, and none have occurred south of latitude 39° except on the Atlantic coast. The cases of low barometer appear to be pretty uniformly distributed along the different meridians until we come within two hundred miles of the Atlantic coast, with but one exception, viz., Fort Sully. There is reason to suspect that in 1874 the readings of the barometer at this station were too low. During the first six months of 1874 the mean height of the barometer at this station was more than one-tenth of an inch below that of the neighboring stations Duluth, Breckenridge and Yankton. If we apply this correction to the observations reported at Fort Sully, the number of cases at this station below 29·25 inches will be reduced to four, which accords very well with the results at other western stations. The observations at Cape Rosier, Father Point and Sydney did not commence until November, 1873, and those at Chatham in October, 1873. There is then a sudden increase in the violence of storms on approaching the Atlantic coast, and this may be ascribed to the increased supply of aqueous vapor coming from the ocean and the Gulf Stream. This increased supply of vapor results in an increased fall of rain; that is, increased expansion of the air, causing an increased violence of the wind, and hence diminished pressure.

In nine of the cases included in the table on page 2, the velocity of the wind was reported zero, and in thirty-one cases the velocity did not exceed five miles per hour, showing that near the center of an area of low pressure there is usually a period during which the air is almost entirely calm.

In fifty-three of the cases included in the table, no rain was reported for the preceding eight hours at the station where the barometer was lowest; and in seventy-eight of the cases the rain-fall was less than one-tenth of an inch, showing that the principal rain-fall does not take place at the center of a low area, but considerably to the east of that center.

An area of low barometer may have considerable progressive motion when there is no rain-fall, as is shown in several examples quoted in my 7th paper; but the abundant rain-fall which usually accompanies great barometric depressions tends greatly to modify the movement of the storm's center. Since this rain-fall takes place chiefly on the east side of the low center, the heat which is liberated in the condensation of the aqueous vapor causes a stronger inflow of air on the east side and a continued fall of the barometer on that side. If there should be a great precipitation of vapor on the west side of the center of least pressure, this must be accompanied by ascending currents of air on that side which would oppose the establishment of the equilibrium of pressure on that side, and the center of the storm may be held stationary or even diverted to the west. This appears to afford the explanation of the phenomenon of stationary storms, of which No. 36 in the table on page 2 is a very remarkable example. This storm traveled from Montana to Nova Scotia at the rate of twenty-four miles per hour; but from March 9th to March 10th, when near Nova Scotia, its motion did not exceed three miles per hour. From the 10th to the 11th its motion was scarcely fifteen miles per hour, and from the 11th to the 14th the center oscillated to and fro, its average rate of progress for the three days being less than three miles per hour. This period was characterized by a heavy fall of snow on the west side of the center of low pressure, as is shown by the following table.

Precipitation, March 9–14, 1874.

	9.1	9.2	9.3	10.1	10.2	10.3	11.1	11.2	11.3	12.1	12.2	12.3	13.1	13.2	13.3	14.1
Rochester	·13	·11	·01	·10	·03		·02	·01		·22	·04	·02	·10	·09	·03	·04
Montreal	·40	·10		·03	·20	·20	·07					·01	·01			
Ottawa	·20	·10		·10	·10		·30	·10								
Quebec	·20		·30	·10	·20	·20	·10					·20				
Father Point		·01	·05	·35	·05	·02				·33						
Cape Rosier	·57	·03	·19	·26	·19			·10	·15	·05		·10	·30	·10		
Chatham		·17	·30	·15									·01	·01		

The date is given at the top of each column, and the precipitation, which was chiefly in the form of snow, is expressed in inches of water. We see that during these five days the average precipitation was more than one inch of water, which corresponds to about one foot of snow, and this snow-fall extended westward 1,000 miles from the center of least pressure, and there was considerable precipitation even beyond these limits. This remarkable precipitation of vapor which continued so long and extended over so large an area on the west side of the low centre, is believed to have been the cause of the slow eastward progress of the storm.

This unusual precipitation on the west side of the storm is ascribed in part to an east wind which prevailed at a moderate elevation above the earth's surface, while a strong wind from the west or northwest prevailed at the surface of the earth. This fact is shown by the following observations of the wind and the upper clouds at Portland, Eastport and Halifax.

Date.	Station.	Wind.	Clouds.
March 11.2	Eastport	N. W.	E.
11.2	Halifax	N. W.	S.
11.3	Halifax	W.	S.
12.1	Eastport	N. W.	E.
12.1	Portland	W.	N. E.

The precipitation on the west side of the low center ceased March 14th, and after that date the low center traveled rapidly eastward. From the 14th to the 15th its average progress was twenty-nine miles per hour, from the 15th to the 16th it was forty-seven miles per hour, from the 16th to the 17th it was thirty-six miles per hour, from the 17th to the 18th it was only thirteen miles per hour, and from the 18th to the 19th when off North Cape it was only eight miles per hour. The progress of this storm from Montana to the North Cape is shown on Chart I, and the isobaric curves show that there was a sudden increase in the violence of the storm on approaching the Atlantic coast.

No. 10 of the table on page 2 presents another example of a nearly stationary storm. From February 22d to the 24th the center of this storm remained for forty-eight hours near Quebec, and for the next two days the storm traveled very slowly, but I have no means of determining the exact position of the low center. The following observations show that at this time there was an unusual precipitation on the west side of the storm's center.

Precipitation, February 22–26, 1873.

	22.1	22.2	22.3	23.1	23.3	25.1	25.3	26.1	Sum
Mt. Washington	·15		·50	·32			·45	·50	1·92
Quebec	·40	·50	·20		·30	·01		·61	2·02

No. 40 of the table on page 2 presents another example of the same kind. The center of this storm remained near Father Point from the morning of April 30th for about two days, and it was four days in reaching St. Johns, Newfoundland. In this case also there was an unusual precipitation of vapor on the west side of the center of low pressure, as is shown by the following table:

Precipitation, April 30–*May* 3, 1874.

	30.1	30.2	30.3	1.1	1.2	1.3	2.1	2.2	2.3	3·1	3.2	Sum
Montreal	·06				·80			·01				·87
Quebec	·50	·40	·50	·30				·10				1·80
Chatham	·21	·02		·12	·06	·02	·31	·12	·06	·04		·96
Cape Rosier	·21	·27	·01			·10	·15	·40	·20	·20		1·54
Sydney	·34	·23								·11	·09	·77

A still more remarkable example of the same kind occurred on the Atlantic Ocean in the winter of 1874–5. A storm of unusual violence extended entirely across the Atlantic, and for many days the center of the storm was nearly stationary. On the 30th of December, 1874, there was a low center (barometer 28·7 inches) a short distance south of Greenland, with an area of high pressure on the east (barometer 30·5 inches) and an area of high pressure on the west (barometer 30·8). On the 31st the low center occupied nearly the same position (barometer 28·3), with a high on the east (bar. 30·7) and a high on the west (bar. 30·8). January 1st the low was nearly stationary (bar. 28·7), with a high on the east (bar. 30·5) and a high on the west (bar. 30·7). January 2d the low had advanced 750 miles eastward (bar. 28·9), with a high on the east (bar. 30·5) but the high on the west had nearly disappeared under the influence of a low area approaching from the west. January 3d the low had advanced nearly 400 miles further east (bar. 28·7), with a high on the east (bar. 30·5) and another low on the west (bar. 28·9) followed on the west side by a high (bar. 30·5). January 4th the two low areas had blended into a single low area (bar. 28·9), and the center of least pressure was thereby transferred about 400 miles westward, with a high on the east (bar. 30·5) and a high on the west (bar. 30·7). There was at this time a subordinate low center (bar. 29·7) further east, and it may be claimed that this was the continuation of the storm of December 30th, but it appears to me more reasonable to consider the great depression of January 4th to be the continuation of that of December 30th. January 5th the low center had moved a little southward (bar. 28·9) with highs both east and west. January 6th the low had moved toward the northwest (bar. 28·9) with highs both east and west. January 7th the low had moved considerably to the south (bar. 28·5) with highs both east and west. January 8th the low was nearly stationary (bar. 28·5) with a high on the east, but the high on the west was nearly broken down by a new area of low pressure which was advancing from the west. January 9th the low area was nearly stationary (bar. 28·7), with a high on the east much reduced (bar. 30·3) and an equal high on the west. January 10th low stationary (bar. 28·7) with a high on the east (bar. 30·5), with a small low on the west (bar. 29·5) and a high further west (bar. 30·6). January 11th the two low areas coalesced and the resulting center of least pressure was thereby carried 650 miles westward (bar. 28·5), with a high on the east (bar. 30·7) and a high on the west (bar. 30·4). In this case there was a subordinate low center (bar. 29·3) about 2,000 miles in a northeast direction, which has the appearance of having been detached from the larger low area since the morning of January 10th, and this may perhaps be claimed as being the continuation of the great storm of the twelve preceding days; but it seems to me most reasonable to regard the greater low area as being the continuation of that of December 30th. January 12th the low was stationary (bar. 27·9), with a high on the east (bar. 30·7) and a high on the west (bar. 30·5). The barometer had now attained its greatest depression, and the area of low pressure (below 30 inches) stretched entirely across the Atlantic Ocean, including the whole of Newfoundland on the west and the whole of Great Britain on the east, extending southward to latitude 32° and northward beyond latitude 70°. Plate II represents the isobars for January 12th and also the path of the storm's center from December 30th to January 18th. January 13th the center was nearly stationary, January 14th it had moved a little eastward, January 15th it had moved a little northward, and during the three following days it moved a little eastward. Thus on January 18th the center of the storm was only 900 miles eastward of its position on December 30th, and it was 200 miles westward of its position January 3d. The principal westerly motion of the storm center appeared to result from the influence of another low center advancing from the west and uniting with the former. This result took place January 4th and again January 11th, and also January 15th.

This entire period of twenty days was characterized by winds of hurricane violence on the western side of the low center, attended by extensive precipitation, which on the western side consisted chiefly of snow and sleet, with very low temperature, the thermometer on the American coast remaining much of the time near zero of Fahrenheit, and at stations but little removed from the coast sinking more than 20° below zero. This precipitation is believed to have taken place chiefly over the ocean, where its amount could not be measured. The following table shows the precipitation in Newfoundland, New Brunswick and its vicinity, and also shows the lowest temperature observed each day at two of the stations in New Brunswick and one in Manitoba.

	Precipitation, Jan. 1–18, 1875.						Lowest Temperature.		
	St. Johns.	Sydney.	Char-lottet'n.	Frede-rickton.	Chat-ham.	Quebec.	Frede-rickton.	Chat-ham.	Winni-peg.
Jan. 1		0·04					— 1°·4	— 0°·5	—16°·6
2		·16	0·35	0·48	0·75	0·40	— 3·3	— 3·0	—27·8
3	1·00	·49	·35	·75	·25		6·5	— 1·0	—26·3
4		·01					— 9·5	— 6·6	—22·9
5	·10	·06	·01				— 3·4	—10·0	—30·4
6							—12·6	—11·3	—29·5
7		·03	·10		·06	·12	— 6·5	—11·3	—24·5
8	1·20	1·55	1·60	·93	1·45	·40	— 6·5	— 5·6	—45·5
9		·10	·15		·28	1·00	—13·1	—15·3	—39·2
10	1·00	·31	·15	·15	1·12		— 1·8	— 4·6	—33·8
11	·10					·01	—20·7	—16·3	—36·0
12	·02						—28·9	—17·0	—36·3
13				·60	·01	·70	—15·8	—14·3	—36·5
14	·25	·36	·55	·02	·64	·30	1·8	0·0	—34·7
15							— 7·5	— 8·8	—29·9
16	·40						— 3·3	0·5	—31·1
17	·30		·09		·09		— 1·8	— 1·3	—34·2
18							— 5·4	— 6·5	—34·8
Sum	4·37	3·11	3·35	2·93	4·65	2·93			

Over the Atlantic Ocean near the parallel of 30°, throughout the month of January the mean height of the barometer is about 30·1 inches. During the twenty days now under examination there were only four days in which the pressure in that region fell below 30·3 inches. Throughout these twenty days there was always an area of high pressure on the east, varying from 30·3 to 30 7 inches, generally at a distance of 1,500 to 2,000 miles. On five days the center of greatest pressure was distant at least 3,000 miles, but there was always a pressure of at least 30·3 inches within a distance of about 2,000 miles. On the west there was always an area of high pressure varying from 30·4 to 30·8 inches, and generally at a distance of about 1,500 miles; but on five days its distance was about 3,000 miles. It is not known whether there was an area of high pressure on the north, but we know that during this period the temperature at the north was extremely low. Throughout this entire period there was therefore a cause to produce a wind from four different directions towards the center of this storm, and the force was sufficient to set the air in motion over an immense area. The usual phenomena attending areas of low barometer must therefore follow, viz: high winds, a fall of the barometer, and a precipitation of vapor. On the present occasion these phenomena were exhibited in unusual intensity, because the center of low pressure was directly over the Gulf

Stream, where the temperature of the water in January is nearly 60°, and the air which flowed in from the north and west was excessively cold. It seems probable that this cold northwest wind pushed under the warmer wind from the southeast as was observed in the storm of March 11, 1874, and that this was one cause of the unusual precipitation on the west side of the storm-center This extensive precipitation on the west side of the low center is believed to have been the principal cause of the slow progress of the storm eastward.

Violent Winds.

In order to determine the laws which govern the velocity of the wind I selected from the published volumes of the Signal Service observations (September, 1872, to May, 1874,) all those cases in which the velocity of the wind amounted to forty miles per hour. The number of these cases was 250, excluding the observations at Mount Washington and Pike's Peak, where high velocities are very common. Of these 250 cases, eighty-two were reported at 7:35 A. M., ninety-one at 4:35 P. M., and seventy-seven at 11 P. M. These results indicate a slight influence of the regular diurnal inequality in the force of the winds, but the inequality depending upon the hour of the day is only one-fifth as great as is found in the average of the daily observations for an entire year; showing that the causes which determine the force of the daily winds have but little influence in controlling the force of the most violent winds.

The following table shows the average number of cases of violent winds for each month of the year:

Month.	Cases.	Month.	Cases.	Month.	Cases.
Jan.	22	May	5	Sept.	3
Feb.	12	June	1	Oct.	6
March	21	July	3	Nov.	23
April	20	Aug.	2	Dec.	9

Thus we see that during the six months, from November to April, violent winds are more than five times as frequent as during the other six months of the year. But the average force of the daily winds for the former period is only one-fourth greater than during the latter period, showing, as before stated, that the causes which determine the force of the daily winds have but little influence in controlling the force of the most violent winds.

The following table shows the number of cases in which the wind blew from each of the eight principal points of the compass at the time of these high velocities. We see that violent winds come from a northern quarter two and a half times as frequently as they do from a southern quarter, and they seldom come directly from the south.

North	43 cases.	South	6 cases.
Northeast	48 "	Southwest	33 "
East	23 "	West	26 "
Southeast	19 "	Northwest	52 "

The following table shows all the stations at which these violent winds were reported in more than two cases, and it also shows the number of cases for each of the eight principal points of the compass:

	N.	N.E.	E.	S.E.	S.	S.W.	W.	N.W.	Sum.
Quebec		33	9			9	4	1	56
Breckenridge	4			9				15	28
Indianola	22	1							23
Fort Sully	6		5	1			1	5	18
Yankton	2	1	2			1	1	8	15
Cape Rosier		3	1	2		4		1	11
Father Point		1	2				3	4	10
Fort Benton						5	1	1	7
Eastport		5	2						7
Escanaba				3	2	1			6
Pembina								5	5
Grand Haven							4		4
Cheyenne							2	1	3
Milwaukee						1	1	1	3
Keokuk				1		1	1		3
Omaha	1				2				3
Knoxville	1					2			3
Fort Gibson						1		2	3
Galveston	2							1	3
Key West		1			1				3

The observations at Cape Rosier and Father Point embrace only a period of seven months. A comparison of all the observations indicates a decided preponderance of high velocities at the more northern stations. In order to determine whether the force of the wind generally increases with an increase of latitude, I determined the mean velocity of the wind at all the Signal Service stations and classified the observations according to the latitude of the stations. The following table shows the results at all the stations north of latitude 30°, except Mount Washington and Pike's Peak:

Latitude.	Number of stations.	Average veloc. miles per hour.
30°–34°	15	7·07
35°–39°	26	8·57
40°–44°	36	9·16
45°–49°	10	8·28

These observations indicate that in North America the average force of the wind increases with the latitude from latitude 30° to latitude 45°. Beyond the parallel of 45° there is apparently a diminution in the force of the wind, but this result may be due to the circumstance that beyond this line all the stations are inland stations, where the force of the wind is generally less than it is upon the Atlantic coast. The average force of the wind is generally greatest at stations on the Atlantic coast, the Gulf of Mexico and the Great Lakes, particularly Lakes Michigan and Erie. The force is least at the interior stations distant from large bodies of water, a result which is naturally ascribed to the resistance to the movement of the air caused by the inequalities of the earth's surface. There is, however, an important exception to this rule between the meridian of 94° and the Rocky Mountains, where the mean force of the wind is greater than it is at the interior stations east of that meridian. Between the meridian of 94° and the Rocky Mountains, the average velocity of the wind is 10·2 miles per hour, while between the meridians of 82° and 94° the average velocity is only 7·5, and at the stations situated directly on the Mississippi river the average velocity is only 7·4 miles. This difference is partly due to the circumstance that beyond the meridian of 94° the face of the country is nearly a plane surface almost entirely destitute of forests, and therefore opposes but little resistance to the free motion of the winds. But there is another circumstance which contributes to the same result. Throughout an extensive region east of the Rocky Mountains the annual rain-fall is very small. The winds of this region are therefore dry winds, while the air of the Mississippi valley contains an abundance of vapor. The difference in the specific gravity of the air over the two localities is sufficient to accelerate considerably the west and northwest winds which sweep over the dry prairies west of the Mississippi river.

The same causes which affect the average velocity of the winds affect also the frequency of occurrence of the most violent winds. The region where violent winds are of most frequent occurrence is near the Gulf of St. Lawrence. It has been mentioned that the observations at Cape Rosier and Father Point embrace a period of only seven months, and should therefore be multiplied by three in order to render them comparable with observations at the other stations. An important reason for the greater violence of the winds in this region, is the greater magnitude of the barometric fluctuations and the unusual contrast which exists between the warm and moist winds from the south, and the cold dry winds from the north.

Beyond the meridian of 94°, where the average force of the wind is greatest, violent winds are also of very frequent occurrence, and these violent winds come from the north four times as frequently as they do from the south. Throughout this region, northerly winds sometimes prevail from British America continuously to the Gulf of Mexico. An example of this kind occurred April 7, 1873, when the wind at Indianola was fifty-four miles per hour, and also November 18th, 1873, when the wind at Indianola was fifty miles per hour. In these and many similar cases, there was an area of high barometer in Dakota, causing northerly winds at all places on the south side of this center from Dakota to the Gulf of Mexico. The frequency of violent winds in this region is ascribed to two causes, viz., the country is generally destitute of forests and the air is uncommonly dry.

The frequency of violent winds at Indianola is specially remarkable. In order to discover the cause of this phenomenon I have prepared the following table, in which column 1st shows the dates at which the winds attained a velocity of forty miles per hour; column 2d shows the velocity observed; column 3d shows the direction of the wind; column 4th shows the height of the barometer; column 5th shows the rain-fall in inches during the preceding eight hours; column 6th shows the temperature of the air at Indianola; column 7th shows the average temperature of the water of the Gulf of Mexico during the same month as derived from Maury's thermal charts; column 8th shows the difference between the temperature of Indianola and the temperature of the water in the Gulf; column 9th shows the height of the barometer at some station north of Indianola; column 10th shows the direction of this station; and column 11th shows its distance in miles from Indianola.

Violent winds at Indianola.

	Wind.		Barom.	Rain.	Thermometer.			High on North.		
	Vel.	Direc.			Ind'a t'mp.	Gulf t'mp.	Differ.	Barom.	Direction.	Dist.
1872, Oct. 7.1	43	N.	29·99	0·13	70°	82°	−12°	30·23	N. 45° W.	1200
Nov. 6.2	40	N.	29·71	·16	57	78	−21	·15	N. 43 E.	750
7.1	41	N.	29·83	·02	53	78	−25	·31	N. 45 W.	1200
14.1	46	N.	30·06	0	41	78	−37	·87	N. 31 W.	1500
14.2	48	N.	30·14	0	49	78	−29	·88	N. 31 W.	1500
Dec. 10.1	40	N.	29·86	2·14	47	74	−27	·65	N. 32 E.	690
1873, Jan. 28.1	43	N.	30·20	·05	30	74	−44	·82	N. 7 E.	750
28.2	46	N.	30·33	0	23	74	−51	·74	N. 7 E.	750
28.3	42	N.	30·42	0	20	74	−54	·72	N. 7 E.	750
Feb. 22.3	48	N.	30·17	0	54	74	−20	·43	N. 10 W.	1100
Mar. 2.1	41	N.	30·24	0	50	75	−25	·93	N. 10 W.	1100
25.1	52	N.	29·91	0	57	75	−18	·76	N. 10 W.	1100
25.3	44	N.	30·32	0	48	75	−27	·44	North	1200
April 7.3	54	N.	29·93	0	48	81	−33	·42	N. 10 W.	1100
15.1	41	N.	30·19	0	56	81	−25	·51	N. 31 W.	1500
Nov. 18.1	50	N.	29·95	0	58	78	−20	·42	N. 10 W.	1100
Dec. 3.3	48	N. E.	30·16	0	57	74	−17	·26	N. 31 E.	300
12.3	40	N.	30·22	0	57	74	−17	·46	N. 45 W.	1200
1874, Jan. 4.1	48	N.	30·12	·15	49	74	−25	·50	N. 4 W.	970
4.2	40	N.	30·25	0	44	74	−30	·47	N. 10 E.	500
April 1.1	44	N.	30·17	0	43	81	−38	·31	N. 10 E.	500
8.1	42	N.	29·70	·08	55	81	−26	·46	North	1400
17.1	40	N.	30·05	0	58	81	−23	·38	N. 32 E.	1300

We see that in each of these cases there was an area of high barometer on the north, and this pressure was generally sufficient to urge the wind with considerable force towards Indianola. We also see that in each case the temperature of the air at Indianola was below that of the water on the south side, and generally very much below it; and moreover the air over the Gulf contained an abundance of vapor. This cause alone would be sufficient to produce a fresh breeze from the north at Indianola.

The reason that violent winds are more common at Indianola than at stations further east on the Gulf of Mexico may be the greater dryness of the air near the Rocky Mountains. The annual rain-fall decreases from the Mississippi River to the Rocky Mountains, and the winds must be dryer near the mountains than they are near the Mississippi River. We find a corresponding increase in the mean velocity of the winds as we advance westward from Mobile. At Mobile the mean velocity of the wind is 6·0 miles per hour; at New Orleans it is 7·3 miles; at Galveston it is 9·3 miles; and at Indianola it is 13·7 miles.

At several of the stations, one of the most noticeable peculiarities of these violent winds is the great predominance of winds from a particular direction, viz: at Quebec they come chiefly from the northeast; at Breckenridge chiefly from the northwest; at Indianola from the north; at Yankton from the northwest, etc. This seems to indicate that the direction of the wind is somewhat modified by the configuration of the surface of the surrounding country.

A high velocity of the wind is not invariably associated with a low barometer. In fifty-seven cases (out of 250) the barometer was above thirty inches; in sixteen cases it was above 30·25 inches; and in three cases it was as high as 30·5 inches. The following are the extreme cases:

No.	Date.	Stations.	Barom.	Wind.		Temp.
				Direc.	Force.	
1	1873, Jan. 27.2	Breckenridge	30·75	N.	40	−17°
2	Jan. 15.2	Quebec	30·56	N. E.	41	7
3	1872, Dec. 22.3	Breckenridge	30·50	N. W.	40	−23

In each of these cases there was an area of low barometer within the limits of the United States, and also an area of high barometer; but in cases No. 1 and No. 3 the pressure at Breckenridge was the highest reported at any station at that hour. In No. 1, however, the barometer at Breckenridge rose ·17 inch and in No. 3 it rose ·20 inch before the succeeding observation, which seems to indicate that at the former date the center of high pressure was northwest of Breckenridge. In

No. 1 there was a difference of temperature of 84° betwee Breckenridge and Florida, and in No. 3 a difference of 89 This was doubtless an important part of the moving for which gave the wind such velocity. Nos. 1 and 3 are remar able examples of the force with which a cold wave often swee over Montana and Dakota, bringing at the same time an e tremely low temperature and a very high barometer. In No. the barometer indicated 30·91 inches at Halifax, and the diffe ence of temperature between Quebec and Florida was 69°.

Barometric Gradient.

I have determined the barometric gradient for each of t 250 cases of high velocity of the wind, and a comparison the results shows that the gradient increases with the latitu of the station. In order to determine the rate of increase, divided the observations into four classes: the first containi all the stations south of latitude 35°; the second containing t stations between latitude 35° and 40°; the third between 4 and 45°; and the fourth containing the stations north of la tude 45°. The following table shows the average results; c umn first shows the average latitude of the stations in ea class, and column second shows the average distance in mil between the isobars drawn for each tenth of an inch differen of pressure:

Latitude	29°·3	Distance	98	miles.
"	37·5	"	66	"
"	42·7	"	62	"
"	47·7	"	50	"

It is readily seen that the gradient increases more rapid than the sine of the latitude, but not so rapidly as the squa of the sine of the latitude. It also appears that the gradient about the same in the Mississippi valley as it is near the Atla tic coast in the same latitude, with the exception of the st tions which have a great elevation. At an elevation of 6,0 feet the gradient is nearly one-third less than it is at the low stations.

The observations also indicate that the gradient is less summer than in winter; but the observations are too few number to furnish a satisfactory value of the gradient in su mer for these high velocities.

The following table shows in miles the average distance b tween the isobars (which are drawn to represent a difference pressure of one-tenth of an inch), corresponding to a veloci of the wind from one to fifty miles per hour, excluding the o servations at stations south of latitude 36°. The average la tude of the stations employed is about 45°. The first part the table is taken from my second paper, page 13.

Relation between the velocity of the wind and the distance between the isobars.

Veloc.	Dist.	Veloc.	Dist.	Veloc.	Dist.	Veloc.	Dist.	Veloc.	Dist.
1	134	11	115	21	97	31	78	41	50
2	134	12	114	22	97	32	75	42	49
3	134	13	109	23	97	33	72	43	48
4	131	14	106	24	97	34	69	44	47
5	128	15	104	25	95	35	66	45	47
6	125	16	100	26	92	36	63	46	46
7	123	17	97	27	90	37	60	47	46
8	122	18	97	28	85	38	57	48	45
9	119	19	95	29	85	39	54	49	45
10	117	20	95	30	81	40	51	50	45

There are thirty-five cases reported of velocities exceeding fifty miles per hour, one of them amounting to seventy-one miles per hour, viz., Yankton, May 29.1, 1873. These observations do not indicate any increase in the gradient above that corresponding to a velocity of fifty miles per hour. This fact seems to indicate that these very high velocities are local and are due to a local cause. The barometric gradient is a very reliable indication of the average velocity of the wind over a large area: but over a small area there may be exceptional velocities which do not sensibly affect the barometric gradient as determined from observations at stations distant 100 miles from each other. Such exceptional velocities may result from a strong upward movement of the air in the neighborhood of the station of observation.

In preparing the materials for this article I have been assisted by Mr. Henry A. Hazen, a graduate of Dartmouth College of the class of 1871.

ART. II.—*Notices of Recent American Earthquakes.* No. 7; by Professor C. G. ROCKWOOD, Jr., College of New Jersey.

IN the following notices, those based upon *single* newspaper reports, and which could not be otherwise verified, are printed in smaller type, and the source of the information is indicated.

For information received I am indebted to John M. Batchelder of Boston, Fred. E. Goodrich of the Boston Post, Principal J. W. Dawson of Montreal, Professor E. T. Quimby of Dartmouth College and others.

1876.—May 10. A shock at Santa Barbara, Cal.—(U. S. Sign. Serv.)

Aug. 16. At 1.15 P. M. The bark Forest Queen experienced a heavy shock of fifteen seconds duration in lat. 41° 55′ N., long. 126° 25′ W., off the southern part of Oregon.—(U. S. Sign. Serv.)

Aug. 16. At 11.25 P. M., at Lower Brule Indian Agency, Dakota T., a shock of seven seconds duration with loud rumbling noise.—(U. S. Sign. Serv.)

Sept. 21. A shock about 11.30 P. M., felt at Newport, R. I. and at Fall River and New Bedford, Mass., and adjacent places. At Fort Adams, R. I., it was reported to be "apparently from east to west and lasting about ten seconds."

Sept. 25. Two distinct and heavy shocks with an interval of about fifteen minutes between them were felt about midnight of 24th and 25th throughout southern Illinois and Indiana, from St. Louis, Mo. to Indianapolis, Ind., and Louisville, Ky. They appear to have been most severe in the valley of the Wabash River, from Mt. Carmel and Friendsville, Ill., to Evansville, Ind. The time given for the second and heavier shock varied from 12 midnight to 12.15 A. M.

Sept. 26. A slight shock reported at Friendsville, Ill.—(U. S. Sign. Serv.)

Oct. 6. Two shocks at San Francisco at 9.20 and 10.08 P. M. The first and heavier, lasting ten seconds, with a motion from N. W. to S. E. was felt also at Oakland, San José and Angel Island.

Nov. 20. A shock at Eastport, Me., at 1 P. M.—(U. S. Sign. Serv.)

Dec. 11. About 7 P. M. at Silver Mountain, Cal., a series of seven shocks were felt within a period of thirty minutes, the first and third shocks being heavier than the others. They were preceded by a rumbling noise and apparently came from the south or southeast.

A slight shock was also reported at 3 o'clock next morning at the same place.

Dec. 12. A slight shock in the City of Charleston, S. C., in the evening.—(U. S. Sign. Serv.)

Dec. 21. A shock at Wytheville, Va., at 10.30 A. M.—(U. S. Sign. Serv.)

1877.—Jan. 10. A slight shock about 1.15 P. M. at Los Angeles, Cal., which at Benedict Cañon near there, was felt as three distinct shocks preceded by a loud report.—(Los Angeles Express, in N. Y. Times.)

Jan. 13. A heavy earthquake about noon, forty-five miles southeast of San Diego, Cal. "The reverberations were from east to west, and extended throughout the mountains to the borders of Cajou Valley."—(N. Y. Times.)

Feb. 17. A heavy shock at Quincy, Plumas Co., Cal., in the morning.

Feb. 18. A distinct shock at 2.20 P. M. at Portland, Me., with rumbling sound.

Feb. 24. The Honolulu Gazette of Feb. 28th reports a submarine volcanic outbreak in Kealakeakaua Bay at 3 A. M., Feb. 24. During the same night a severe earthquake was felt at Koawalsa and Kell.

March 8. Two light shocks about 2 A. M., and a few minutes before 5 A. M. at Helena, Montana.—(Helena Independent, in N. Y. Times.)

March 19. A shock at Kingston, Jamaica, at 12.55 A. M.

April 17. A slight shock of short duration at Panama at 5.50 A. M.—(U. S. Sign. Serv.)

April 23. A slight shock at Auburn, N. H., at 11 A. M., from N. W. to S. E.—(U. S. Sign. Serv.)

April 26. A slight shock at Franklin, N. C., at 5 P. M.—(U. S. Sign. Serv.)

May 2. A shock "lasting eight or ten seconds" at 10.20 P. M. at Oshawa, Ontario.

May 9. At 8.30 P. M., a series of severe shocks, lasting four or five minutes, and followed by a destructive tidal wave, were felt on the coast of Peru, Bolivia and Chili. The center of disturbance appeared to be the neighborhood of the volcano Ilaga, on the borders of Peru and Bolivia, in lat. 21° S. The shocks were felt along the coast from beyond Arequipa, Peru, to Valparaiso, Chili, but were most violent and destructive from Iquique, where the duration was 4 m. 20 sec., to Cobija, Mexillones and Chavanago, Bolivia. The disturbance does not appear to have extended very far inland. The sea wave however spread over the entire Pacific, being felt along the coast of Mexico and California (whence it was reported by the signal service a day before news of the shock was received), at the Sandwich Islands, the Marquesas Islands, Japan and Australia. On the Peruvian and Bolivian coasts the wave was from twenty to sixty feet high and caused great destruction of life and property, both on land and among the shipping in the harbors. At the Sandwich Islands the wave was fifteen to thirty-six feet high, and on the Australian coast one to six feet high. Its average rate of progress, as calculated from the time of arrival of the first wave, and assuming its origin to have been near Iquique, was as follows:—to Callao 228 miles per hour, to San Francisco 348 miles, to Honolulu 408 miles and to Australia 378 miles per hour.

On the Peruvian coast lighter shocks continued at intervals for some days, and later reports indicate that the whole coast has suffered a considerable permanent upheaval.

At San José, Costa Rica, a feeble shock was felt at 5.28 A. M. of the 10th (local time) or 9^h 54^m after the shock at Iquique.

In the Sandwich Islands the volcano of Kilauea exhibited unusual activity, and eruptions of lava with occasional earthquake shocks occurred during the week preceding the tidal wave, especially one at 2.45 P M., May 4th.

More detailed local reports of the phenomena may be found in this Journal, III, xiv, p. 77, U. S. Weather Review, May, 1877, Scientific American, vol. xxxvii, pp. 3, 345, Nature, vol. xvi, pp. 112, 174, 198.

May 11. A slight shock in the afternoon in Schenectady and Schoharie Counties, N. Y.

May 14. A severe shock at Lima and Callao, lasting twenty-two seconds.

May 15. From Port Stanley, Ont., it is reported that a wave five feet high, apparently due to some earthquake shock, swept along the northern shore of Lake Erie, followed for an hour by smaller ones.

May 25. A shock at Knoxville, Tenn.—(U. S. Sign. Serv.)

May 26. A shock at 3 P. M. at New Harmony, Ind.—(U. S. Sign. Serv.

May 30. A heavy shock between 2 and 3 A. M., at Pasa Robles, Cal.

June 11. A volcanic eruption, smoke and boulders, in the mountains near Flowing Wells Station, Southern Pacific R. R., about sixty miles from Yuma, preceded by a violent vibration of the earth.

June 18. At Milwaukee, Wis., at 7.30 P. M., the water in Lake Michigan fell two feet in half an hour and rose again more quickly.—(U. S. Sign. Serv.)

June 23. A shock about 11.30 P. M. at Bakersfield, Cal. "It seemed to be in the nature of an upheaval rather than vibratory."

July 9. A shock at Sacramento, Cal., lasting one minute. Oscillations, E. and W.—(U. S. Sign. Serv.)

July 14. Two shocks at 6.40 P. M., at Memphis, Tenn., lasting several seconds, vibrations from S. W. or W. to N. E. or E.

July 15. Three shocks apparently from the west, and lasting five seconds, at Carbondale, Ill.—(U. S. Sign. Serv.)

July 17. A sharp shock at 3 A. M., lasting about thirty seconds, at River du Loup, Canada.

Aug. 10. A shock lasting several seconds at Florence, N. J.—(U. S. Sign. Serv.)

Aug. 17. A slight shock about 11 A. M., felt in Detroit, Mich., and a few neighboring towns. It lasted from thirty seconds to one minute, and was accompanied by a rumbling sound.

———. **At 7.30 P. M. of the same day a heavy shock, lasting fifteen seconds, was felt at Campo, Cal.—(U. S. Sign. Serv.)**

Aug. 23. Shocks of an alarming nature were felt at Cobija, Bolivia, at 1.40 P. M., and at Iquique at 5 P. M., and a few days earlier at Copiapo, Chili.

Sept. 1. A slight shock at 11 A. M. at Latonsville, Sandy Springs, Brookville, Laurel and other points in Prince George's Co., Md.

Sept. 7. A shock at 10 P. M. at Yuma, Arizona.—(U. S. Sign. Serv.)

Sept. 10. A shock at 9.59 A. M., felt in the valley of the Delaware River from Trenton to Philadelphia. It affected an extent of country, about thirty-five miles along the river by twenty miles wide, and having its center at Burlington, N. J. It lasted thirty or forty seconds, had an apparent motion toward the southwest, and was accompanied by a rumbling noise.

Sept. 18. Panama advices of this date say, shocks of earthquake were continuously felt in some of the southern ports of Peru.

Sept. 29. A shock at Campo, Cal., at 2.30 P. M., lasting five seconds, with low rumbling.—(U. S. Sign. Serv.)

Oct. 9. A severe shock at 2.20 A. M. at Lima and Callao, Peru; felt also in Pisco, Ica and Chincha, where two shocks were reported. The vibration was from north to south.

Oct. 12. Quite severe shocks were felt in Oregon, occurring in Portland at 1.53 P. M., two shocks being noticed; at Marshfield, Clackamas Co., at 1.45 P. M.; and at Cascades at 1.52 P. M. (Another shock was felt at Cascades at 9 A. M.) The vibrations were in each case from north to south and were sufficiently violent to overthrow chimneys.

———. At 3.26 A. M. of the same day a slight earthquake was felt on the Isthmus of Panama.

Oct. 26. Between 5 and 6 P. M. the schooner Leo felt a severe earthquake shock, continuing about ten seconds, in lat. 43° 13′ N., long 128° W., the vessel being 300 or 400 miles from the coast of Oregon.

Nov. 4. About 2 A. M. a rather severe earthquake was felt throughout a large part of Canada, New York and New England. It was reported from Ottawa, Perth and many other places in Ontario, east of a line joining Kingston and Pembroke; from Cornwall, Montreal and other places in the St. Lawrence Valley as far east as Three Rivers; from Hanover, N. H., Springfield, Mass., Hartford, Conn., and other places along the Connecticut River; from Burlington and Bennington, Vt., Plattsburg, Whitehall and Saratoga, N. Y., and the valley of Lake Champlain and the Hudson as far south as Albany; and finally from Utica, Rome, Auburn and the Mohawk Valley. It was probably felt throughout the whole Adirondack region which is thus enclosed, but whence no reports could be expected. It would thus seem to have been felt over an irregular trapezium, whose angles are marked by Pembroke, Ont., Three Rivers, P. Q., Hartford, Conn., and Auburn, N. Y.; and which is therefore some 200 miles on its northern and southern sides, about 300 on the east and 175 on the west. In regard to the exact time of the shock the reports differ too much among themselves, and are mostly of too vague a nature to permit any deductions therefrom as to the direction or rate of progress of the vibration. Comparing the reports from thirty-six localities in which the time is given, and rejecting three as quite wide of the truth, we find them cluster closely about 2 A. M., none earlier than 1.45, none later than 2.10, most being between 1.50 and 2, local time. The only ones, however, which appear to be reliably accurate are Montreal 1.50 A. M. (J. W. Dawson), Hartford 1.56 (Ed. Hartford Courant)=1.52 Montreal time, and Dudley observatory, Albany, 1.53 = 1.54 Montreal time. The Hartford record is also confirmed by an apparently careful report from Windsor, Conn., giving 1.55 A. M. Many (25) reports give in a general way "2 A. M.," "about 2 A. M.," etc. The duration in Montreal was about twenty seconds. The other reports of its duration vary from four or five seconds to two or three minutes and in one case five minutes. Most of them however give about half a minute. It seems to have been most severe in the valley of the St. Lawrence and about Lake Champlain, where the vibration was sufficient to overturn crockery, crack ceilings and in a few cases to throw down chimneys. The reports are nearly unanimous that the vibration advanced from west to east, only one report (Hanover, N. H.) stating it as from north to south, this however on the concurrent testimony of three observers. In some places a rumbling noise and in others two or several shocks were reported; at Dudley observatory "a severe shock of ten seconds duration, followed after an interval of thirty seconds by a lighter one;" at Hanover, "a succession of five or six waves, increasing in intensity for the first third of the time and then decreasing;" in Montreal "a low rumbling sound followed by a sharp explosion and then a tremor."

In compiling the above I have been able to compare reports from fifty-eight stations.

Nov. 14. A slight shock at 9.40 A. M. at Cornwall, Ont.

Nov. 15. About noon several shocks were felt in Iowa and adjoining portions of Kansas, Nebraska and Dakota. The times given were: Iowa City 12.30 P. M., Council Bluffs 12.15, movement E. to W., Omaha 11.40 A. M., three shocks lasting ten seconds, Topeka and Atchison, Ks., 12 M., and Yankton, D. T., 11.30 A. M.

These times are plainly discordant, but at this distance no data are available for correcting them.

Nov. 16. A slight shock about 2.20 A. M. at Knoxville, Tenn.

Nov. 18. A shock about 5 A. M. in the Bermuda Islands—(Newark Daily Advertiser).

Princeton, N. J., Nov. 30, 1877.

ART. III.—*Observations on under-water Oceanic Temperature;* by Captain G. E. BELKNAP, U. S. Navy.

[When, many years ago, it was announced by Lieutenant Maury and others, that a cold stratum might exist in the ocean between two adjacent warmer ones, or *vice versa* and that there might be a succession of such conditions between the surface and the bottom, the assertion was believed by many to be fallacious and contrary to the laws of ocean physics, and that warm water would necessarily rise until it was above that of a lower temperature. More recently, however, reliable observations of the northern seas have established this fact, the Germans, the Swedes and the Norwegians having severally verified it. The investigations of Professor Hind on the coast of Labrador, have shown many cases of a similar character. A similar observation was made by Captain Belknap, of the U. S. Navy, while in command of the U. S. steamer Tuscarora, during her famed cruise in the Pacific; and by the courtesy of Commodore Ammen, chief of the Bureau of Navigation, we are permitted to present an abstract of a report made by that officer on the 2d of September, 1874.—EDS.]

A STUDY of the surface and under-surface temperatures, in the Pacific Ocean, has yielded some interesting results. Great care was taken to make the observations as accurate as possible, and the surface temperatures are invariably means of four and five thermometers. Whenever there was any doubt, or any marked change in temperature, the observations were always repeated. In taking the under-surface temperature along the Kurile Islands, the indications of the Miller-Casella thermometers were sometimes so startling and perplexing, the indicated temperatures being so low, that the observations were often duplicated to verify their accuracy; if the second observation confirmed the first, the results were accepted as true; otherwise they were rejected.

Proceeding in this way, a cold stratum between two warm strata, was found to exist quite near the surface, between the parallels of 49° and 52° north latitude and the meridians of 158° and 167° east longitude. The upper part of the stratum in one place, showing a temperature of 33°·7 F., was only 20 fathoms below the surface, while at 10 fathoms below the surface the temperature was 41° F. At a depth of 100 fathoms the temperature was 32° F.; below that curve to a depth of 200 fathoms, the range of temperature was from 34°·5 F. to 38°·7 F. The width of the cold stratum gradually narrowed to a point in an easterly direction from the coast, or as the edge of the Japan stream was approached.

I may say here that the indications of the Miller-Casella thermometers are not always reliable, as the needles sometimes jump in the tubes, and this was especially the case on this northern line, where the changes of under-surface temperatures are so much more sudden than in the Central North Pacific where serial temperatures were taken by this ship.

In three or four instances, the thermometers came up from the bottom and intermediate depths, indicating temperatures of from 20° F. to 28° F.; temperatures manifestly impossible.

There can be no doubt, however, of the existence of the cold stratum described above, as no temperature was recorded until repeated observations with different thermometers confirmed its accuracy.

An inspection of the surface-temperature chart will show that the coldest surface water was found along the Kurile Islands, between the parallels of 45° and 47° north latitude. The temperature of the air was also lower in that region than at any point further north, and the lowest snow line was observed there, though the range of latitude traversed was nearly eight degrees higher. The current in that locality was found to be setting in a southerly direction, and undoubtedly pouring out of the Sea of Ochotsk.

The surface temperatures along the Aleutian Islands show considerable variation, both in the Pacific and in Behring's Sea, doubtless attributable to tidal influences, the direction of the wind, and the meeting of different currents.

In passing from Behring's Sea to the Pacific, through Onnimak Pass, on the morning of the 17th of August, the surface temperature rose seven degrees in one hour, some five to seven miles inside of the Pass, the current at the time setting with moderate strength into the sea.

The lowest surface temperature along the Kurile group was 36°·4 F.; along the Aleutian Islands the surface temperatures ranged from 41°·6 F. to 49° F. and along the continuation of the line, across the Gulf of Alaska, to Cape Flattery, the range of temperature was found to be from 50° F. to 59° F., indicating in the most unmistakable manner the influence of that branch of the Japan stream which, curving to the northward near the meridian of 140° west longitude, impinges upon the coasts of Alaska, and following the line of the shore bordering on the Gulf of Alaska, sweeps back to the westward and southwestward to the Aleutain chain; for the moment Onnimak Pass is cleared, the temperature of the water rises, and the current is found to be setting to the southwestward.

The following table will show some of the differences of temperature observed on the line sounded from Honolulu to the Bonin Islands, and that part of the northern line from the southern point of Kamtchatka to Onnimak Pass in Behring's Sea:

			Southern Line, Central North Pacific.		Northern Line, North Pacific and Behring's Sea.	
Surface,................			70° to	76° F.	36°·4 to	49° F.
Under Surface	30	f'ms,	70°·8	73°·5	31°·5	41°·9
"	50	"	63°·5	72°·7	31°·9	39°·7
"	100	"	62°	70°	31°·1	38°·5
"	200	"	48°·5	58°·7	34°·3	37°·9
"	300	"	41°·4	50°·3	34°·4	37°·3
"	500	"	38°·9	40°·6	34°·8	35°·9
"	1000	"	35°·5		33°·5	33°·8
"	2000	"	33°·2	33°·9	33°·5	33°·8
"	3000	"	33°·2	33° 9	33°·1	33°·3

The very low temperatures noted at the depth of 30 fathoms to 206 fathoms are some of those observed in the cold stratum already described in this report.

The serial temperatures observed on this northern line were not so extended as could have been wished, owing to the loss and breakage of thermometers and to the fact that two of the instruments furnished had to be thrown aside as worthless. In the latter case the mercury would crowd up past the needle, rendering the indications valueless, and that defect in those instruments could not be overcome.

The thermometers sent down at the 3,664 fathom cast came up unharmed, and as some of the "Challenger's" instruments broke at a depth of 3,875 fathoms the maximum pressure which the Miller-Casella thermometers will bear must be somewhere between those depths, or a pressure of about four and three-fourths tons per square inch.* The temperature indicated at 3,664 fathoms is 33°·8 F.

I should take into consideration, however, the fact that using hempen line and time intervals for sounding, that the depths found by the "Challenger" were not so accurate as those obtained by the "Tuscarora" with piano wire and dynamometer.

* From later results obtained by the Challenger this deduction is probably incorrect, as I believe she received whole thermometers back on one or two occasions from a depth of ovor 4,000 fathoms.

ART. IV.—*On the Magnetic Effect of Electric Convection;*[*] by HENRY A. ROWLAND of the Johns Hopkins University, Baltimore.

THE experiments described in this paper were made with a view of determining whether or not an electrified body in motion produces magnetic effects. There seems to be no theoretical ground upon which we can settle the question, seeing that the magnetic action of a conducted electric current may be ascribed to some mutual action between the conductor and the current. Hence an experiment is of value. Professor Maxwell, in his "Treatise on Electricity," Art. 770, has computed the magnetic action of a moving electrified surface, but that the action exists has not yet been proved experimentally or theoretically.

The apparatus employed consisted of a vulcanite disc 21·1 centimeters in diameter and ·5 centimeter thick which could be made to revolve around a vertical axis with a velocity of 61· turns per second. On either side of the disc at a distance of ·6 cm. were fixed glass plates having a diameter of 38·9 cm. and a hole in the center of 7·8 cm. The vulcanite disc was gilded on both sides and the glass plates had an annular ring of gilt on one side, the outside and inside diameters being 24·0 cm. and 8·9 cm. respectively. The gilt sides could be turned toward or from the revolving disc but were usually turned toward it so that the problem might be calculated more readily and there should be no uncertainty as to the electrification. The outside plates were usually connected with the earth; and the inside disc with an electric battery, by means of a point which approached within one-third of a millimeter of the edge and turned toward it. As the edge was broad, the point would not discharge unless there was a difference of potential between it and the edge. Between the electric battery and the disc, a commutator was placed, so that the potential of the latter could be made plus or minus at will. All parts of the apparatus were of non-magnetic material.

Over the surface of the disc was suspended, from a bracket in the wall, an extremely delicate astatic needle, protected from electric action and currents of air by a brass tube. The two needles were 1·5 cm. long and their centers 17·98 cm. distant from each other. The readings were by a telescope and scale. The opening in the tube for observing the mirror was protected from electrical action by a metallic cone, the mirror being at its vertex. So perfectly was this accomplished that no effect of electrical action was apparent either on charging the battery or reversing the electrification of the disc. The needles were so far apart that any action of the disc would be many fold greater on the lower needle than the upper. The direction of the needles was that of the motion of the disc directly below them, that is, perpendicular to the radius drawn from the axis to the needle. As the support of the needle was the wall of the laboratory and revolving disc was on a table beneath it, the needle was reasonably free from vibration.

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[*] The experiments described were made in the laboratory of the Berlin University through the kindness of Professor Helmholtz, to whose advice they are greatly indebted for their completeness. The idea of the experiment first occurred to me in 1868 and was recorded in a note book of that date.

In the first experiments with this apparatus no effect was observed other than a constant deflection which was reversed with the direction of the motion. This was finally traced to the magnetism of rotation of the axis and was afterward greatly reduced by turning down the axis to ·9 cm. diameter. On now rendering the needle more sensitive and taking several other precautions a distinct effect was observed of several millimeters on reversing the electrification and it was separated from the effect of magnetism of rotation by keeping the motion constant and reversing the electrification. As the effect of the magnetism of rotation was several times that of the moving electricity, and the needle was so extremely sensitive, numerical results were extremely hard to be obtained, and it is only after weeks of trial that reasonably accurate results have been obtained. But the qualitative effect, after once being obtained, never failed. In hundreds of observations extending over many weeks, the needle always answered to a change of electrification of the disc. Also on raising the potential above zero the action was the reverse of that when it was lowered below. The *swing* of the needle on reversing the electrification was about 10· or 15· millimeters and therefore the point of equilibrium was altered 5 or 7½ millimeters. This quantity varied with the electrification, the velocity of motion, the sensitiveness of the needle, etc.

The direction of the action may be thus defined. Calling the motion of the disc + when it moved like the hands of a watch laid on the table with its face up, we have the following, the needles being over one side of the disc with the north pole pointing in the direction of positive motion. The motion being +, on electrifying the disc + the north pole moved toward the axis, and on changing the electrification, the north pole moved away from the axis. With − motion and + electrification, the north pole moved away from the axis, and with − electrification, it moved toward the axis. The direction is therefore that in which we should expect it to be.

To prevent any suspicion of currents in the gilded surfaces, the latter, in many experiments, were divided into small portions by radial scratches, so that no tangential currents could take place without sufficient difference of potential to produce sparks. But to be perfectly certain, the gilded disc was replaced by a plane thin glass plate which could be electrified by points on one side, a gilder induction plate at zero potential being on the other. With this arrangement, effects in the same direction as before were obtained, but smaller in quantity, seeing that only one side of the plate could be electrified.

The inductor plates were now removed, leaving the disc perfectly free, and the latter was once more gilded with a continuous gold surface, having only an opening around the axis of 3·5 cm. The gilding of the disc was connected with the axis and so was at a potential of zero. On one side of the plate, two small inductors formed of pieces of tin-foil on glass plates, were supported, having the disc between them. On electrifying these, the disc at the points opposite them was electrified by induction but there could be no electrification except at points near the inductors. On now revolving the disc, if the inductors were very small, the electricity would remain nearly at rest and the plate would as it were revolve through it. Hence in this case we should have conduction without motion of electricity, while in the first experiment we had motion without conduction. I have used the term "nearly at rest" in the above, for the following reasons. As the disc revolves the electricity is being constantly conducted in the plate so as to retain its position. Now the function which expresses the potential producing these currents and its differential coefficients must be continuous throughout the disc, and so these currents must pervade the whole disc.

To calculate these currents we have two ways. Either we can consider the electricity at rest and the motion of the disc through it to produce an electromotive force in the direction of motion and proportional to the velocity of motion, to the electrification, and to the surface resistance; or, as Professor Helmholtz has suggested, we can consider the electricity to move with the disc and as it comes to the edge of the inductor to be set free to return by conduction currents to the other edge of the inductor so as to supply the loss there. The problem is capable of solution in the case of a disc without a hole in the center but the results are too complicated to be of much use. Hence scratches were made on the disc in concentric circles about ·6 cm. apart by which the radial component of the currents was destroyed and the problem became easily calculable.

For, let the inductor cover $\frac{1}{n}$ the part of the circumference of any one of the conducting circles; then, if C is a constant, the current in the circle outside the inductor will be $+\frac{C}{n}$, and inside the area of the inductor $-C\frac{(n-1)}{n}$. On the latter is superposed the convection current equal to +C. Hence the motion of electricity throughout the whole circle is $\frac{1}{n}$, what it would have been had the inductor covered the whole circle.

In one experiment n was about 8. By comparison with the other experiments we know that had electric conduction alone produced effect we should have observed at the telescope −5· mil. Had electric convection alone produced magnetic effect we should have had +5·7 mil. And if they both had effect it would have been +·7 mil., which is practically zero in the presence of so many disturbing causes. No effect was discovered, or at least no *certain* effect, though every care was used. Hence we may conclude with reasonable certainty that electricity produces nearly if not quite the same magnetic effect in the case of convection as of conduction, provided the same quantity of electricity passes a given point in the convection stream as in the conduction stream.

The currents in the disc were actually detected by using inductors covering half the plate and placing the needle over the uncovered portion; but the effect was too small to be measured accurately. To prove this more thoroughly numerical results were attempted, and, after weeks of labor, obtained. I give below the last results which, from the precautions taken and the increase of experience, have the greatest weight.

The magnetizing force of the disc was obtained from the deflection of the astatic needle as follows. Turning the two needles with poles in the same direction and observing the number n of vibrations, and then turning them opposite and finding the number n' of vibrations in that position, we shall find, when the lower needle is the strongest,

$$X-X'\frac{n^2-n'^2}{n^2+n'^2}=\frac{n'^2}{n^2+n'^2}\,\frac{\Delta}{D}\,H. \qquad (1)$$

Where X′ and X are the forces on the upper and lower needle respectively, Δ the deflection, D the distance of the scale and H the horizontal component of the earth's magnetism. As X′ and n' are very small the first term is nearly X−X′. The torsion of the silk fiber was too small to affect the result, or at least was almost eliminated by the method of experiment.

The electricity was in the first experiment distributed nearly uniformly over the disc with the exception of the opening in the center and the excess of distribution on the edge. The surface density on either side was

$$\sigma = \frac{V - V'}{2\pi(B - \beta)}. \qquad (2)$$

$V - V'$ being the difference of potential between the disc and the outside plates, β the thickness of the disc and B the whole distance apart of the outside plates. The excess on the edge was (Maxwell's Electricity, Art. 196, Eq. 18),

$$E = 2(V - V')\frac{BC}{\pi(B - \beta)}\log_e\left(2\cos\frac{\pi\beta}{2B}\right), \qquad (3)$$

where C is the radius of the disc.

We may calculate the magnetic effect on the supposition that, as in the conducted current, the magnetizing force due to any element of surface is proportional to the quantity of electricity passing that element in a unit of time. The magnetic effect due to the uniform distribution has the greatest effect. With an error of only a small fraction of a per cent, we may consider the two sides of the disc to coincide in the center. Taking the origin of coördinates at the point of the disc under the needle and the center of the disc on the axis of X we find for both sides of the disc, the radial component of the force parallel to the disc,

$$X = \frac{8\pi N\sigma a}{v}\int_{-(C+b)}^{C-b}\int_0^{r'}\frac{(b+x)dxdy}{(a^2+x^2+y^2)^{\frac{3}{2}}} =$$

$$\frac{8\pi N\sigma a}{v}\int_{-(C+b)}^{C-b}\frac{(b+x)\sqrt{C^2-(b+x)^2}}{(a^2+x^2)\sqrt{a^2+C^2-b^2-2bx}},$$

where a is the distance of the needle from the disc and b that from the axis; N is the number of revolutions of the disc per second and $v = 28{,}800{,}000{,}000$ centimeters per second according to Maxwell's determination. The above integral can be obtained exactly by elliptic integrals, but as it introduces a great variety of complete and incomplete elliptic integrals of all three orders, we shall do best by expanding as follows:

$$X = \frac{4\pi N\sigma}{v}P = \frac{4\pi N\sigma}{v}(A_1 + A_2 + A_3 + \&c.), \qquad (4)$$

$$A_1 = 2b\left(\text{arc tan}\,\frac{C-b}{a} + \text{arc tan}\,\frac{C+b}{a}\right) - a\log_e\frac{M}{N},$$

$$A_2 = -\frac{a}{2b}\left((s+b)\log_e\frac{M}{N} - 2C\right),$$

$$A_3 = \frac{a}{16b^2}\left\{-4Cs + (3s^2+2sb+a^2)\log_e\frac{M}{N}\right.$$

$$\left. + \left(5s^3+3s^2b+a^2(s+b)\right)\frac{4Cb}{MN}\right\} \&c., \&c.,$$

where $s = \frac{a^2+C^2-b^2}{2b}$, $M = a^2+(C+b)^2$, $N = a^2+(C-b)^2$.

From this must be subtracted the effect of the opening in the center, for which the same formula will apply.

The magnetic action of the excess at the edge may be calculated on the supposition that that excess is concentrated in a circle of a little smaller diameter, C′, than the disc;

therefore, $$X_1 = \frac{NE}{v} \frac{ak}{2b\sqrt{C'b}} \left\{ \frac{2-k^2}{1-k^2} E(k) - F(k) \right\}, \qquad (5)$$

where $k = \dfrac{2\sqrt{C'b}}{\sqrt{a^2+(C'+b)^2}}$, and F($k$) and E($k$) are complete elliptic integrals of the second and first orders respectively.

The determination of the potential was by means of the spark which Thomson has experimented on in absolute measure. For sparks of length l between two surfaces nearly plane, we have on the centimeter, gram, second system, from Thomson's experiments,

$$V-V' = 117{\cdot}5(l+{\cdot}0135),$$

and for two balls of finite radius, we find, by considering the distribution on the two sheets of an hyperboloid of revolution,

$$V-V' = 117{\cdot}5\,(l+{\cdot}0135)\frac{r}{2\sqrt{r+1}} \log_e \frac{\sqrt{r+1}+1}{\sqrt{r+1}-1},$$

where r is the ratio of the length of spark to diameter of balls and had in these experiments a value of about 8. In this case

$$V-V' = 109{\cdot}6\,(l+{\cdot}0135). \qquad (6)$$

A battery of nine large jars, each 48· c. m. high, contained the store of electricity supplied to the disc, and the difference of potential was determined before and after the experiment by charging a small jar and testing its length of spark. Two determinations were made before and two after each experiment, and the mean taken as representing the potential during the experiment.

The velocity of the disc was kept constant by observing a governor. The number of revolutions was the same, nearly, as determined by the sizes of the pulleys or the sound of a Seebeck siren attached to the axis of the disc; the secret of this agreement was that the driving cords were well supplied with rosin. The number of revolutions was 61· per second.

In such a delicate experiment, the disturbing causes, such as the changes of the earth's magnetism, the changing temperature of the room, &c., were so numerous that only on few days could numerical results be obtained, and even then the accuracy could not be great. The centimeter, gram, second system, was used.

First Series. $a=2{\cdot}05$, $b=9{\cdot}08$, $n={\cdot}697$, $D=110{\cdot}$, $H-{\cdot}182$ nearly, $B=1{\cdot}68$, $\beta={\cdot}50$, $C=10{\cdot}55$, $N-61{\cdot}$, $v=28{,}800{,}000{,}000{\cdot}$, $n'={\cdot}0533$, $C'=10$.

Direction of motion.	Electrification of disc.	Scale reading in mm.	Deflection on reversing electrificat'n in mm.	Length of spark.
+	−	99·	7·25	·295
	+	107·5		
	−	101·5		
−	+	68·5	8·25	·290
	−	76·5		
	+	68·0		
+	+	97·	7·00	·282
	−	91·5		
	+	100·		
−	+	59·	6·75	·265
	−	65·5		
	+	58·5		
+	+	92·5	6·75	·290
	−	85·		
	+	91·0		
−	+	52·5	5·50	·285
	−	57·5		
	+	51·5		
+	+	82·0	5·85	·285
	−	76·0		
	+	81·7		
−	+	36·5	6·50	·275
	−	43·0		
	+	36·5		
+	+	68·0	7·00	·290
	−	61·0		
	+	68·0		
−	+	27·5	6·50	·288
	−	33·5		
	+	26·5		
Mean values.			6·735	·2845

Hence, $\Delta=\frac{\cdot 6735}{2}=\cdot 337$ and $l=\cdot 2845$.

From equation (1),

$$X-\cdot 99\,X'=\frac{1}{305700\cdot}=00000327.$$

By calculation from the electrification we find

$$X-\cdot 99\,X'=\frac{1}{296800\cdot}=\cdot 00000337.$$

The effect on the upper needle, X′, was about $\frac{1}{50}$ of that on the lower X.

Second Series. Everything the same as before except the following. $b=7\cdot 65$, $n'=\cdot 0525$.

Direction of motion.	Electrification of disc.	Scale reading in mm.	Deflection on reversing electrificat'n in mm.	Length of spark.
+	+	172·5	7·0	·300
	—	165·5		
	+	172·5		
—	+	120·0	7·5	·295
	—	127·5		
	+	121·5		
	—	129·0		
+	—	163·5	7·25	·297
	+	170·5		
	—	163·0		
	+	170·5		
—	+	118·0	8·25	·270
	—	127·0		
	+	120·0		
	—	127·5		
Mean values.			7·50	·2955

$$\therefore \quad \Delta = \cdot 375, \qquad l = \cdot 2955.$$

ence for this case we have from equation (1),

$$X - \cdot 99\, X' = \frac{1}{315000^{\cdot}} = \cdot 00000317.$$

nd from the electrification,

$$X - \cdot 99\, X' = \frac{1}{286000^{\cdot}} = \cdot 00000349.$$

Third Series. Everything the same as in the first series, cept $b = 8{\cdot}1$, $n' = \cdot 0521$, $D = 114$.

Direction of motion.	Electrification of disc.	Scale reading in mm.	Deflection on reversing electrificat'n in mm.	Length of spark.
—	+	151·0	7·50	·287
	—	158·5		
	+	151·0		
+	+	192·0	7·25	·292
	—	185·5		
	+	193·5		
—	—	157·5	8·25	·295
	+	148·5		
	—	157·5		
	+	150·0		
+	—	185·0	7·75	·302
	+	192·5		
	—	185·5		
	+	193·5		
—	—	151·0	7·25	·287
	+	143·5		
	—	150·5		
Mean values.			7·60	·2926

$$\therefore \quad \Delta = \cdot 380, \qquad l = \cdot 2926.$$

For this case from equation (1)

$$X - \cdot 99\, X' = \frac{1}{295000} = \cdot 00000339,$$

and from the electrification

$$X - \cdot 99\, X' = \frac{1}{281500} = \cdot 00000355.$$

The error amounts to 3, 10 and 4 per cent respectively in the three series. Had we taken Weber's value of v the agreement would have been still nearer. Considering the difficulty of the experiment and the many sources of error, we may consider the agreement very satisfactory. The force measured is, we observe, about $\frac{1}{50000}$ of the horizontal force of the earth's magnetism.

The difference of readings with + and − motion is due to the magnetism of rotation of the brass axis. This action is eliminated from the result.

It will be observed that this method gives a determination of v, the ratio of the electromagnetic to the electrostatic system of units, and if carried out on a large scale with perfect instruments might give good results. The value $v = 300{,}000{,}000$ meters per second satisfies the first and last series of the experiments the best.

Berlin, February 15th, 1876.

ART. V.—*Notes of Observations on Jupiter and its Satellites;* by MARIA MITCHELL.

THE following observations were made at the observatory of Vassar College; longitude $4^h\ 55^m\ 33^s$, latitude 41° 41′ 18″. The instrument used was the Equatorial telescope; the power usually 230.

1874, May 2.—Observations on Jupiter began at $10^h\ 18^m$. The seeing was excellent. With a power of 600 the ruddiness of the equatorial belt was brought out; two large dark spots on its upper portion were very striking in appearance, the intercepted space between them being conspicuously white. The shadow of the 4th satellite was near egress. It touched the limit of the planet, in internal contact at $10^h\ 37^m\ 10^s{\cdot}3$, and was last seen at $11^h\ 2^m\ 45^s{\cdot}8$. At times during the hour I thought the shadow was followed by a companion shadow.

1874, May 3.—The 3d satellite of Jupiter was occulted at $9^h\ 11^m\ 2^s{\cdot}5$. [This observation is by Miss Fisher (student) with a small telescope.]

1874, May 7.—The ingress of the 1st satellite of Jupiter was bserved. The satellite was in external contact at $10^h\ 50^m\ 39^s$. The internal contact was at $10^h\ 55^m\ 7^s$.

1874, May 14.—Observations on Jupiter began at 8 P. M. The broad equatorial belt was rosy and was seen fully out to he following limb. The shadow of the 3d satellite was upon he disc. The shadow was dark but not black. I could not call it circular; the longer diameter was nearly parallel with he equatorial belt. It left the planet's disc at $10^h\ 1^m\ 11^s{\cdot}1$.

1874, May 19.—The noticeable peculiarity of the planet's disc is that of a large white spot on the broad belt near the center at $8^h\ 10^m$ P. M. At $9^h\ 45^m$ no trace of this spot could be found, although the equatorial belt was seen out to the preceding limb.

1874, May 22.—There was a very decided change in the spots from dark to light between $9^h\ 6^m$ P. M. and $9^h\ 35^m$ P. M.

1874, May 28.—The planet was unusually striped. The broad belt was much spotted and its upper part heavily shaded. There was a rosy tinge over the whole belt. The 3d satellite ouched the limb of Jupiter, at ingress, at $9^h\ 56^m\ 44^s$ P. M. Its nternal contact was at $10^h\ 6^m\ 15^s$ P. M. Although it entered on a part of the planet which was not bright, it could be folowed only ten minutes.

1874, June 2.—There were peculiar white markings on the ower part of the equatorial belt of Jupiter; these were beyond the center at $8^h\ 25^m$. The 2d satellite touched the limb in occultation at $8^h\ 2^m\ 14^s{\cdot}3$; was wholly occulted at $8^h\ 9^m\ 42^s{\cdot}3$.

1875, April 11.—Observations on Jupiter began at midnight. The night was fine and the planet was near the meridian. The 1st satellite touched the limb in ingress at $0^h\ 37^m\ 52^s$ A. M. The internal contact with limb was at $0^h\ 46^m\ 16^s$. The satellite was last seen $1^h\ 2^m\ 51^s$, although it was upon the dark belt. The shadow was followed for some six minutes longer.

1875, April 23.—The 3d satellite of Jupiter was wholly off from the disc of the planet at $7^h\ 15^m\ 2^s$ P. M. The shadow of the 3d satellite touched the limb, at egress, at $7^h\ 43^m\ 7^s$ P. M. The shadow was wholly off at $8^h\ 1^m\ 51^s$ P. M. Measurements were made of both satellite and shadow. The diameter of the satellite measured $2''{\cdot}17$, of the shadow $1''{\cdot}95$.

1875, April 30.—The 3d satellite touched the following limb of Jupiter at $8^h\ 21^m\ 19^s{\cdot}3$. The internal contact was at $8^h\ 40^m\ 24^s{\cdot}3$. The shadow of the satellite was fully upon the disc at $9^h\ 48^m\ 9^s{\cdot}3$. The equatorial belt was slightly ruddy, and dark and white spots could be seen upon it. The 1st and 4th satellites were so nearly of the same size that I could distinguish them only by position.

1875, May 10, 8 to 9.30 P. M.—A white loop extended diagonally over more than half the equatorial belt of Jupiter.

1875, June 2.—Observations began at $7^{h}\ 55^{m}$ P. M. A very remarkable white spot was at once seen, nearly at the center of Jupiter's disc. It was followed by a very dark shading, so that it strongly resembled a satellite and shadow in transit. The white spot was so well defined as to be easily measured. The longer diameter (oblique to equatorial belt) was $1''\cdot7$. At 9 P. M. the white spot was approaching the limb of the planet, turning apparently with the planet, and was followed by one smaller and less distinct.

1875, June 4.—Observations began at $7^{h}\ 30^{m}$ P. M. At $7^{h}\ 54^{m}$ no white spot could be seen. At $8^{h}\ 15^{m}$ the white spot with the peculiar appearance of shadow following could be seen, as on June 2. It was, at this time, $\frac{1}{3}$ of the diameter of Jupiter, distant from the following limb. At $9^{h}\ 15^{m}$ the second white spot was seen, following the first, as on June 2. The 1st satellite touched the limb of Jupiter at $8^{h}\ 21^{m}\ 24^{s}$ P. M. The internal contact was at $8^{h}\ 26^{m}\ 19^{s}$ P. M. The satellite entered upon the planet below the broad belt, very white, more brilliant than the spot, but smaller and scarcely more conspicuous. The shadow of the satellite was first seen upon the disc at $9^{h}\ 23^{m}\ 44^{s}$ P. M.

1876, May 30.—Observations began at $9^{h}\ 39^{m}$ P. M. The broad belt on the disc of Jupiter was mostly above the equator. It was mottled with large white spots, somewhat rose-tinged. The 1st satellite as it approached the planet was of a dazzling whiteness; it entered upon the disc above the lower margin of the equatorial belt, yet it could be seen for only twelve minutes. The shadow entered on the disc about sixteen minutes later very black, but not round, the longer diameter being nearly perpendicular to equatorial belt. 1st satellite touched limb of Jupiter at $9^{h}\ 53^{m}\ 26^{s}$ P. M.; was at internal contact $9^{h}\ 59^{m}\ 22^{s}$. The shadow was wholly on the disc at $10^{h}\ 15^{m}\ 12^{s}$ P. M. The satellite was seen at $10^{h}\ 11^{m}\ 32^{s}$ P. M. It is possible that the satellite was faintly seen at $10^{h}\ 34^{m}\ 2^{s}$.

1876, May 31.—The peculiarity in the appearance of Jupiter is the presence of bright white spots on the upper portion of the disc, markings resembling faculæ on the Sun. The first satellite was seen to come out of eclipse at $9^{h}\ 44^{m}\ 41^{s}\cdot99$.

1876, June 15.—Observations on Jupiter began at $8^{h}\ 20^{m}$ P. M. The first satellite was known to be in transit, but could not be seen upon the disc. The shadow of the 1st satellite was wholly within the limb at $9^{h}\ 34^{m}\ 51^{s}\cdot9$. At $9^{h}\ 55^{m}\ 36^{s}\cdot9$ the 3d satellite was seen to come out from occultation. At $9^{h}\ 58^{m}\ 11^{s}\cdot9$ the 3d satellite was wholly out and shining with brilliant white light. At $9^{h}\ 20^{m}\ 42^{s}$ the 1st satellite was found, approaching the preceding limb, a dull gray figure, elliptic in shape, the major axis being perpendicular to equatorial belt. When this satellite reached the limb it was as white as the 3d satellite, and round. The transit was made across the brightest part of the disc, and where there were no perceptible variations of brilliancy. The 1st satellite's last contact with limb was at 10^h 2^m 47^s.

1876, June 22.—Observations began at 10^h 12^m P. M. The 1st satellite, known to be upon the disc, could not be found. The shadow was wholly on at 10^h 26^m 56^s. The satellite was seen, dusky, oval and gray, from 10^h 55^m 33^s to 11^h 10^m 33^s.

1877, June 13.—The shadow of the 2d satellite was seen, wholly entered upon the disc, at 11^h 20^m 45^s. The satellite itself touched the limb at 11^h 36^m 07^s; was at internal contact at 11^h 43^m 30^s.

1877, June 19.—The 3d satellite touched the limb of the planet at 10^h 47^m 53^s. The internal contact was at 10^h 59^m 10^s. The 1st satellite reappeared from occultation at 11^h 4^m 51^s. The ruddiness of the equatorial belt was noticed by several observers.

ART. VI.—*Revision of the Atomic Weight of Antimony;* by JOSIAH P. COOKE, Jr.

[**Abstract of a paper, in the Proceedings of the American Academy of Arts and Sciences, xiii, 1, prepared by the Author.**]

IN 1856, R. Schneider of Berlin made a very careful determination of the atomic weight of antimony and obtained for its value 120·3. His method consisted in reducing native antimony glance by means of hydrogen and his investigation was a model of its kind. In his paper* all the details of the experimental work are given and it is evident that every precaution was taken which the circumstances required. In 1857, Dexter by oxidizing the metal with nitric acid obtained for the same constant the much higher value 122·3,† and soon after this result was apparently closely confirmed by Dumas, whose analyses of antimonious chloride gave almost precisely 122.‡ The present investigation was undertaken with the view of reconciling if possible the large discrepancy between the results of these equally careful and accurate experimenters. It was suggested by the method, devised by the author, of precipitating sulphides which was described in a previous number of this Journal.§ This seemed to afford a very accurate method of making the synthesis of antimonious sulphide and thus of revising the analysis of Schneider. In carrying out the method, however, unexpected difficulties were encountered which are discussed at length in the original paper. Two of these arose from the circumstance that the antimonious sulphide as usually precipitated occludes a small amount of tartaric acid on the one hand and of oxichloride of antimony on the other. These occlusions tend to produce errors in opposite directions, for when, before weighing, the red antimonious sulphide is heated to the point of its conversion into the gray modification, the tartaric acid is charred, and the carbonaceous residue increases the apparent weight of the product; while on the other hand the oxichloride of antimony is decomposed at the same temperature and the antimonious chloride, which volatilizes, tends to diminish the weight of the product. In the earlier determinations these causes of error were balanced as nearly as possible by regulating the conditions of the precipitation; but it was subsequently found to be possible to entirely prevent the occlusion of antimonious oxichloride, and in all the later determinations allowance was made for the small amount of carbonaceous residue, which was accurately estimated in every case.

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* Poggendorff's Annalen, xcviii, 455, June, 1856.

† Ibid, c, 563, April, 1857.

‡ Ann. Chem. et de Phys., III, lv, 175, Feb., 1859.

§ This Journal, III, xiii, 427.

In the preparation of pure metallic antimony, we were greatly guided by the experience of Mr. Dexter;* and our several products must have been very similar to his, as the following determinations of the specific gravities of the different buttons show. The observed values were reduced to 4° C., on the assumption that the coefficient of cubic expansion for antimony between 0° and 100° C. is for each degree 0·000033, as observed by Kopp. The letters here given will be used throughout the table to designate the various specimens. As might be supposed, the specimens were prepared at different times and at different stages of the investigation, but the results are united here for the convenience of comparison and of reference.

SPECIFIC GRAVITIES OF BUTTONS OF PURE METALLIC ANTIMONY.

Observations of J. P. C., Jr.		Observations of W. Dexter.	
A	6·7025	b	6·7087
B	6·7036	c	6·7026
C	6·6957	c	6·6987
D	6·7070	d	6·7102
E	6·7022	e	6·7047
F	6·7023	e	6·7052
Mean	6·7022	Mean	6·7050

The processes used in preparing the several buttons just referred to, the method by which the metal was brought into solution in its lowest condition of quantivalence, and the manner in which the antimonious sulphide was precipitated, collected, dried and weighed are all described at length in the original paper. The following table shows the results which were obtained.

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* Poggendorff's Annalen, c, 564 (*l. c.*).

SYNTHESIS OF SULPHIDE OF ANTIMONY.

No.	Wt. in grams of Sb taken.	Wt. dissolved from balls.	Total weight of Sb used.	Wt. of red Sb_2S_3 dried at 180° C.	Per cent. of S in same.	Corresponding At. Wt. of Sb when S=32.	Wt. of black Sb_2S_3 dried at 210° C.	Per cent. of S in same.	Corresponding At. Wt. of Sb when S=32.
1. A.	2·0036	0·2023	2·2059	3·0898	28·61	119·79	3·0881	28·57	120·02*
2. E.	2·0017	0·2662	2·2679	3·1778	28.63	119·64	3·1764	28·60	119·82*
3. E.	2·0113	0·0853	2·0966	2·9383	28·65	119·56	2·9350	28·57	120.03*
4. A.	1·9973	0·0798	2·0771	2·9051	28·50	120·41	2·9021	28·43	120·85†
5. E.	2·0019	0·1087	2·1106	2·9508	28·47	120·57	2·9486	28·42	120·89†
Mean of 5 Determinations			-----	-----	28·572	119·994	-----	28·518	120·32
6. A.	1·7638	0·0430	1·8068	2·5301	28·59	119·91	-- --	-----	-----‡
7. A.	2·0275	0·0894	2·1169	2·9639	28·57	119·97	-----	-----	... .‡
8. B.	2·0116	0·0188	2·0304	2·8423	28·57	120·04	2·8410	28·53	120·23§
9. B.	2·0027	0·1000	2·1027	2·9429	28·55	120·13	2·9409	28·50	120·41§
10. E.	2·0015	0·1424	2·1439	3·0025	28·58	119·94	2·9981	28·49	120·47§
11. E.	2·0038	0·3379	2·3417	3·2792	28·59	119·90	3·2788	38·58	119·95§
12. E.	2·0014	0·2168	2·2182	3·1061	28·59	119·92	3·1022	28·50	120·44§
Mean of last 5 Determinations			-----	-----	28·576	119·986	-----	28·520	120·298
13. D.	2·0056	0·3787	2·3843	3·3369	28·55	120·14	3·3369	28·51	120·14‖
Mean of the 13 Determinations			-----	-----	28·5731	119·994	-----	28·522	120·295

* Large residue of carbon, small sublimate.

† Small residue of carbon, large sublimate.

‡ Weight of black sulphide not found.

§ Both residue and sublimate small.

‖ No sublimate or loss of weight on drying and conversion. Residue large, but weighed and subtracted. The best determination, and as perfect as can be made under conditions.

We were for some time in doubt in what condition the sulphide of antimony ought to be weighed, in order to obtain the most accurate results. Our final judgment was that the errors already referred to would best be balanced, while others would be avoided, by weighing the sulphide, after it had been dried, at from 180° to 200°, but before it was actually converted into the gray sulphide. This conversion takes place between 210° and 220°, varying to that extent in different cases. The change, as we infer, is attended with a sudden evolution of heat, and the action is quite violent. Small particles of the material are frequently projected from the vessel, and we sometimes noticed that the surface of the platinum nacelle became coated with the familiar sublimate of sulphide of antimony. If there is oxichloride in the precipitate, there may be an additional volatilization of chloride of antimony at this time; but the main loss, as we have constantly observed, takes place before the point of conversion is reached. We therefore concluded that more trustworthy results could be deduced from the weight of the red sulphide dried, as we have described, than from that of the gray; and, as will be seen, this judgment was fully confirmed by subsequent experiments on the haloid compounds. We have, however, in all but two instances weighed the sulphide in both conditions, and we give the results of both weighings; and on comparing these results in determinations eight to thirteen inclusive of the table on page 43, which were made under the nearly identical conditions we have above indicated, it will be seen that the differences are far smaller with the red sulphide than with the gray, which shows conclusively that additional causes of error must have affected the last weights, —a circumstance which sustains our judgment.

In the first twelve determinations we did not estimate the amount of the carbonaceous residue, which is assumed to be balanced by the loss of chloride of antimony, although we always tested the purity of the sulphide of antimony by dissolving it in hydrochloric acid, as described. In determination numbered thirteen, we succeeded in precipitating the antimony without the usual occlusion of oxichloride. In this case, there was no evidence of sublimation nor loss during conversion, but a proportionally large carbonaceous residue, which was deducted from the weight of the sulphide; and the result of this determination, as will be seen, still further corroborates our conclusion. The same is true of the analyses of chloride of antimony made more recently, in which we dissolved crystallized chloride of antimony in a concentrated aqueous solution of tartaric acid, without using any excess of hydrochloric acid. In these cases also, the drying of the precipitate, and the conversion from the red to the gray modification, were attended with no appearance of sublimation. Were we to repeat the investigation with our present knowledge, we should follow the indications of these last analyses; and instead of attemping to make the two chief errors as small as possible, and balance them, we should seek to remove from the solution all the free hydrochloric acid, and thus eliminate the error due to the occlusion of oxichloride. It would then, of course, be necessary to determine in all cases the carbonaceous residue, which might however be very large, without impairing the accuracy of the result. Still, our experience with these antimony determinations would lead us to fear that we might thus raise up as many hindrances as we avoided, and the determination we have given as No. 13 is sufficient for all purposes of comparison.

This point in our investigation was reached in the spring of 1876, and the results given above were presented to the American Academy of Arts and Sciences at their meeting of June 14th, 1876. But although they agreed so closely with the results of Schneider, and although the close confirmation of his analysis thus furnished by our synthesis seemed so conclusive, yet we could not rest satisfied so long as the great discrepancy between this value of the atomic weight and the higher number obtained by Dumas remained unexplained. We therefore determined to repeat his experiments before publishing our results. Accordingly, in the autumn of 1876, we purified and analyzed a large number of different specimens of antimonious chloride, and the results are united in the following table. Beginning with crystallized chloride of antimony obtained from different dealers, and pure in a commercial sense, we first boiled for several hours the melted chloride over finely pulverized metallic antimony, using for the purpose a glass retort, so tilted that the condensed liquid flowed back into the body of the vessel. When boiled in this way, the surface of the vapor is marked by a very well-defined ring in the neck of the retort; and by regulating the lamp this ring can readily be maintained very near the mouth, so that, while all the chloride of antimony condenses and flows back, any more volatile admixtures will gradually escape. The retort having been brought into its normal position, the chloride of antimony was next distilled; and, rejecting the first and last eighth which came over, the rest of the product was redistilled over strips of metallic zinc, and so on three or four times, rejecting at each distillation the first and last of the product. The final distillate was then still further purified by repeated crystallizations from fusion. As the fused mass solidifies quite slowly (indicating a large loss of latent heat), it is easy to arrest the process at any point, and pour off the still liquid portion from the crystals which have formed. The last can then be remelted, and the process repeated, and so on indefinitely as long as the material lasts. In this way, from several kilograms of the commercial chloride we obtained the few grams of beautifully clear and perfect crystals used in our analyses. In the fifth preparation, the crystals were obtained not by fusion, but by cooling a saturated solution of the previously distilled chloride in purified sulphide of carbon. Such a solution, saturated at the boiling point of sulphide of carbon, deposits the larger part of the chloride, when cooled to the ordinary temperature. Naturally, every precaution was taken during the course of these preparations to protect this exceedingly hygroscopic substance from contact with moist air, and all the transfers were made in a portable photographic developing chamber, the air of which was kept dry by dishes of sulphuric acid. The portions for analysis were transferred, in this chamber, to tightly fitting weighing tubes; and, after the weight was taken, they were dissolved in a concentrated aqueous solution of tartaric acid, using about five grams of tartaric acid to each gram of chloride of antimony. The solutions were then diluted, and precipitated with argentic nitrate, weighing out in each case the amount required, so that only the least possible excess of the reagent should be added. The precipitates were washed and collected by reverse filtering in platinum or porcelain crucibles, and dried in an air bath at temperatures varying from 110° to 120°. They were weighed with the small disk of paper used in this process, and the invariability of the weight of these paper disks was repeatedly tested. Also, in several instances after removing the filter, the argentic chloride was heated to incipient melting; but, as in no case was its weight thus altered, this additional precaution seemed unnecessary. In the determination numbered 17, an attempt was made to ascertain whether the presence of antimony in the tartaric acid solution appreciably influences the precipitation of argentic chloride. In this analysis, the antimony was first separated from the solution by H_2S; and, the excess of this reagent having been removed by warming the filtrate with a small amount of ferric nitrate, the chlorine was precipitated in the usual way. The results, as will be seen, agree as nearly as could be expected with those obtained by at once precipitating the chlorine from the antimony solution."

The letters in the following table indicate different preparations, thus:—

a Was made from a crystallized chloride of Veron and Fontaine, Paris, which was purified in the manner described above.

b Was made from a crystallized chloride marked Rousseau Frères, Paris, purified as before.

c Was the same as *b*, again distilled and crystallized.

d, The same as *c*, after ten additional distillations.

e, The same as *d*, again distilled below 100° in a current of dry hydrogen gas.

f Was made with a crystallized chloride from Merck of Darmstadt, purified by repeated distillations and subsequent crystallizations from bisulphide of carbon, after first treating with chlorine as described beyond.

g, Same as *f*, but in this determination the antimony was first precipitated from the solution.

ANALYSIS OF ANTIMONIOUS CHLORIDE.

DETERMINATION OF CHLORINE.

No.	$SbCl_3$. grams.		AgCl. grams.	% of Chlorine. Cl=35·5. Ag=108.	At. Wt. of Sb. Cl=35·5.
1 *a.*	1·5974	yielded	3·0124	46·653	121·78
2 *a.*	1·2533	“	2·3620	46·623	121·93
3 *a.*	0·8876	“	1·6754	46·696	121·57
4 *b.*	0·9336	“	1·5674	46·516	122·46
5 *b.*	0·5326	“	1·0021	46·446	122·30
6 *b.*	0·7270	“	1·3691	46·588	122·10
7 *c.*	1·2679	“	2·3883	46·599	122·04
8 *c.*	1·9422	“	3·6646	46·678	121·66
9 *c.*	1·7702	“	3·3384	46·655	121·77
10 *d.*	2·5030	“	4·7184	46·635	121·87
11 *d.*	2·1450	“	4·0410	46·616	121·96
12 *e.*	1·7697	“	3·3281	46·524	122·42
13 *e.*	2·3435	“	4·4157	46·613	121·98
14 *f.*	1·3686	“	2·5813	46·659	121·75
15 *f.*	1·8638	“	3·5146	46·650	121·79
16 *f.*	2·0300	“	3·8282	46·653	121·78
17 *g.*	2·4450	“	4·6086	46·630	121·89
	Mean value for all analyses			46·620	121·94
	Theory when Sb = 122			46·608	122·
	“ “ Sb = 120			47·020	120·

If in calculating the per cent of chlorine from the results of the above determinations we use the atomic weights for silver and chlorine obtained by Stas (namely, Cl=35·457 and Ag=107·93), these per cents will be in each case very nearly 0·020 lower, and we shall obtain for the mean value 46·600 instead of 46·620. Moreover, on this assumption the atomic weight of antimony, deduced from Dumas's analysis of the chloride, would be 121·95 instead of 122. Again, if we use Stas's value of the atomic weight of sulphur (S=32·074) in calculating the atomic weight of antimony from our own results, on the synthesis of the sulphide, we should obtain 120·28 instead of 120; and, lastly, the values Sb=120·28 and Cl=35·457 give for the per cent of chlorine in antimonious chloride the value 46·931.

Here, then, is a most striking result: for these determinations confirm the value of the atomic weight of antimony obtained by Dumas as closely as did the previous determinations confirm that obtained by Schneider. Evidently, there was a large constant error in one case or the other. Moreover, it appeared improbable that in either case any error could arise in the chemical process employed: for, in the first instance, we had a synthesis by one method confirming an analysis by a wholly different method; and, in the second instance, the analytical process employed is regarded as one of the most accurate known to science, and we had apparently shown that its accuracy was not impaired under the peculiar conditions present. It appeared, therefore, reasonable to assume that the results did truly indicate both the actual proportion of antimony in the sulphide of antimony and of chlorine in the chloride of antimony analyzed, and to look for the cause of the discrepancy to some impurity in one or the other compound. We therefore next sought to determine how much sulphide of antimony could be obtained from a given weight of chloride of antimony, hoping that by thus bringing the relations of antimony to chlorine and sulphur into close comparison the source of the error might be indicated.

The following table exhibits the results of these antimony determinations, as well as the general result of the assumed complete analysis of antimonious chloride. The per cent of chlorine taken is the mean of the first thirteen determinations of the previous table, as these only had been made at the time the second table was drawn up, and it therefore exhibits the results exactly as they were presented to us at this stage of the investigation.

ANALYSIS OF ANTIMONIOUS CHLORIDE.

DETERMINATION OF ANTIMONY.

	$SbCl_3$ taken in grams.	Sb_2S_3 obtained in grams.	% of Antimony when Sb : S=120 : 32.*	% of Antimony if Sb : S=122 : 32.†
1 *b*.	3·8846	2·8973	53·275	53·525
2 *b*.	5·1317	3·8417	53·473	53·725
3 *b*.	4·4480	3·3201	53·316	53·567
4 *b*.	4·5506	3·4009	53·882	53·633
5 *b*.	4·8077	3·6072	53·593	53·845
6 *b*.	4·2774	3·1958	53·367	53·618
Mean of all Analyses			53·401	53·652

MEAN RESULTS OF COMPLETE ANALYSIS.

Antimony, the mean of six determinations	53·401	53·652
Chlorine, " " thirteen "	46·611‡	46·611‡
	100·012	100·263

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* Or assuming that $\frac{5}{7}$ of the gray sulphide is antimony, as deduced from actual synthesis.

† According to the generally accepted theory.

‡ When Cl=35·5 and Ag=108, according to Dumas.

As they at first presented themselves to us, these new results, so far from throwing light on the subject, only rendered the problem the more obscure and baffling. Towards interpreting them, however, one point seemed evident:—that, however little value our own experiments and those of Schneider might have in fixing the atomic weight of antimony, they had at least established, beyond all doubt, the proportion of this element in the gray sulphide weighed in our antimony determinations. For if we assumed, as those experiments indicated, that five-sevenths of the gray sulphide was antimony, then the amounts of antimony and chlorine found in the analysis of antimonious chloride just made almost exactly supplemented each other; while on the other hand, if this material was, as generally believed, pure Sb_2S_3, in which $Sb:S=122:32$, then our determinations of one or the other of these elements must be greatly erroneous, and the excess obtained far too great to be explained by any known or probable imperfections of our methods. Of course, although the gray sulphide might contain, on the average, five-sevenths of its weight of antimony, it was a possible supposition that it might also occlude a constant amount of some undiscovered impurity, leaving the proportion of the sulphur to the antimony that which the atomic weights 122 and 32 required; and, were it not for our previous experience, this would have been the most obvious explanation of the discrepancy. Indeed, the new facts led us to re-examine this material, and review our previous conclusions, but with the same result as before. We could discover no impurity except the small amount of carbonaceous material which was well known and taken into the account, and in our later determination even this had been reduced to so small an amount as to be wholly insignificant.

[To be continued.]

ART. VII.—*On a new mineral, Pyrophosphorite: an Anhydrous Pyrophosphate of Lime from the West Indies;* by CHARLES UPHAM SHEPARD, Jr., Professor of Chemistry in the Medical College of the State of South Carolina.

THROUGH the kindness of Mr. C. C. Wyllie of London, England, I have been put in the possession of a few small fragments of a mineral phosphate from a new locality in the West Indies. Commercial considerations forbid at present the publication of the precise position of this deposit, but later I hope to be able to announce it, as also to give information with regard to the extent, mode of occurrence, and so on.

The mineral is generally snow-white and opaque, with here and there a slight tinge of bluish-gray. The white portion is dull and has an earthy fracture like magnesite; the grayish—constituting perhaps one-third of the mass—is small-botryoidal like gibbsite, and is somewhat the harder of the two.

The specific gravity varies between 2·50 and 2·53; hardness between 3 and 3·5. Before the blowpipe it melts with difficulty on the edges to a whitish enamel.

The following are the results of several chemical analyses of the mineral, as executed on two different portions of material:

	1st Series.	2d Series.	Mean.
Loss on ignition	0·390	......	0·390
Lime	44·50	44·424	44·462
Magnesia	3·04	3·141	3·090
Sulphuric acid	0·57	0·687	0·628
Phosphoric acid	50·97	50·629	50·799
Silica	0·30	0·434	0·367
Oxide of iron and alumina	....	0·437	0·437
			100·173

If we add together the adventitious ingredients, viz: the pyrophosphates of iron and alumina (taking an equal portion of phosphoric acid as the oxide of iron preponderates), the sulphate of lime, silica and loss on ignition, we obtain:

	Per cent.
Pyrophosphates of iron and alumina	0·874
Sulphate of lime	1·068
Silica	0·367
Loss on ignition	0·390
	2·699

There remain,

	Per cent.			
Lime	44·022	or	45·16	on raising the 97·474 per cent. to 100 per cent.
Magnesia	3·090		3·17	
Phosphoric acid	50·362		51·67	
	97·474		100·00	

The above composition agrees with the formula

$$2MgO, P_2O_5+4(5CaO, 2P_2O_5)^* \text{ or } 2MgO, P_2O_5+4\left\{\begin{array}{l}3CaO,P_2O_5\\2CaO,P_2O_5\end{array}\right.$$

which would require the following amounts of

	By calculation.	Actually found.
Lime	45·20 per cent.	45·16 per cent.
Magnesia	3·23	3·17
Phosphoric acid ..	51·57	51·67
	100·00	100·00

* Expressed according to atomic system $Mg_2P_2O_7+4\left\{\begin{array}{l}Ca_3P_2O_8\\Ca_2P_2O_7\end{array}\right\}$

The mineral therefore is (essentially) anhydrous ortho-pyrophosphate of lime with pyrophosphate of magnesia. The absence of water naturally suggests that its repository must have been in contact with some igneous formation. The proportion of phosphoric acid exceeds that of any known calcic phosphate, and it is likewise distinguished by its excess of phosphoric acid over the bases, in conjunction with almost total absence of water of chemical combination. This latter property separates it distinctly from the class of phosphates represented by the Mejillones and Raza deposits.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Direct Combustion of Nitrogen.*—Kämmerer has described an interesting lecture experiment to show the direct union of the nitrogen and oxygen of air. In a cylinder or globe of about two liters capacity filled with air, a piece of burning magnesium ribbon about thirty to forty centimeters long, is placed. When the combustion of the magnesium is ended, an intense odor of nitrogen tetroxide is perceived, and when the magnesium oxide has deposited, the characteristic color of this gas may be observed. By pouring into the jar a solution of potassium iodide acidified with acetic acid, and agitating, the brown color of free iodine at once appears, which strikes a deep blue color when starch solution is added.—*Ber. Berl. Chem. Ges.*, x, 1684, Oct., 1877. G. F. B.

2. *On the Relative Attraction of Oxygen for Hydrogen and Carbonous oxide.*—Bunsen, in his "Gasometric Methods," has given the results of an examination of the products formed when hydrogen and carbonous oxide are mixed with oxygen in quantity insufficient to burn them both completely, and exploded; from which he draws the conclusion that the atomic ratio of the combustion products ($H_2O:CO_2$) is always represented by simple numbers, changing from one to the other *per saltum* as the proportion of hydrogen varies. Horstmann has experimented anew in this direction and has obtained quite different results. He finds: 1st. That when electrolytic gas and carbonous oxide are mixed and exploded, following the method used by Bunsen, the proportion of the former gas being continually increased, vapor of water and carbon dioxide are formed in a constantly increasing ratio. Thus while the hydrogen, relatively to the carbonous oxide, varied from 0·25 : 1 to 2·33 : 1, from 20 to 70 per cent of the mixture being burned, the ratio of the combustion-products ($H_2O:CO_2$) varied from 0·8 : 1 to 4·5 : 1. Nothing like a sudden change of ratio was anywhere observed. 2d. That when to a mixture of carbonous oxide and hydrogen, increasing quantities

of oxygen are added and the whole exploded, as in E. v. Meyer's method, aqueous vapor and carbon dioxide are formed in a constantly increasing ratio, no "preference for rational values" being detected. Hence the author maintains that the division of the oxygen between these two combustible gases does not take place according to the law laid down by Bunsen. As to the cause of the discrepancy, he says that while his experiments were made with dry gases in dry tubes, he has observed that when aqueous vapor is present in the tube, less hydrogen and more carbonous oxide is burned. The ratio of $H_2O : CO_2$ is always lessened, and the more, the greater the amount of moisture present. On the other hand, in presence of carbon dioxide, more hydrogen and less carbonous oxide is burned. Now v. Meyer's experiments were made with moist gases, and Bunsen's partly with dry and partly with moist gases. Moreover, the variation in the ratio of the combustion-products is peculiar. As the oxygen increases, this ratio at first increases, reaches a maximum, when the amount of combustible gases burned is about 30 to 35 per cent, and then falls uniformly again to the limiting value reached when the combustion is complete. The law according to which the oxygen divides itself between the combustible gases, is thus expressed: The ratio of the aqueous vapor formed to the carbon dioxide is equal to the ratio of the unburned hydrogen to the unburned carbonous oxide, multiplied by an affinity-coefficient, which is independent of the proportion in which the combustible gases are present, but which varies with the relative quantity of oxygen added. The relation between this coefficient and the necessary quantity of oxygen being fixed empirically in the case of one proportion of such a mixture, it may be readily calculated for all others. This coefficient, in Horstmann's experiments, varied from 4·0 to 6·4, when between 20 and 70 per cent of the combustible gases were burned, the maximum being reached at 30 to 40 per cent. In other words, the ratio of the aqueous vapor to the carbon dioxide was 4 to 6·4 times as great as the ratio of the hydrogen to the carbonous oxide in the unburned residue. Since therefore relatively more hydrogen is always burned than carbonous oxide, the attraction of oxygen is greater for the former than for the latter gas. The variability of the affinity-coefficient itself is due to the fact that with the relative quantities of oxygen, the physical conditions change under which the reaction takes place. With a fixed quantity of oxygen it remains constant, not only when the proportion of hydrogen to carbonous oxide varies, but even when the unburned portions of the combustible gases are wholly or partially replaced by an indifferent gas with similar properties; as, for example, nitrogen.—*Ber. Berl. Chem. Ges.*, x, 1626, Oct., 1877. G. F. B.

3. *On the Sulphides of Platinum.*—RIBAN has called attention to the analytical properties of platinic and platinous sulphides. From his experiments he concludes, 1st, that platinic sulphide prepared in the cold or at the temperature of the water bath,

when taken alone, or at least in the absence of the metals of the 5th and 6th groups of Fresenius, may be considered as insoluble in alkali or ammonium mono- or polysulphides, and hence should be classed with mercury in the fifth group; 2d, that while by various special devices, such as pouring a solution of platinic chloride into a sulphide, drop by drop, or in fusing platinic sulphide with an alkali sulphide, a considerable quantity of platinic sulphide may be dissolved, yet that this is not a condition occurring in ordinary analysis; 3d, that in presence of metals of the sixth group, platinic sulphide dissolves notably in alkali or ammonium polysulphides. Arsenic, antimony, tin and gold effect this result, the quantity of platinum sulphide dissolved increasing with the amount of the other sulphides present; 4th, that platinic sulphide when mixed with the fifth group of metals, is not dissolved by the monosulphide, but is sensibly acted on by the trisulphide of ammonium, thus resembling copper; 5th, that platinous sulphide may be considered soluble or insoluble according to its physical state and the nature of the sulphide used. Platinous salts occur rarely in analysis, however, and then are easily converted into platinic compounds.—*Bull. Soc. Ch.*, II, xxviii, Oct., 1877.

G. F. B.

4. *On the Destructive Distillation of Phenol and Chlorbenzene.*—**Kramers** has examined the decomposition products obtained when phenol and chlorbenzene are distilled at a bright red heat. The phenol was allowed to flow slowly into a horizontal gas pipe kept at a bright red in a Hofmann's furnace. The products received were fractionated and yielded benzene, toluene, xylene, naphthalene, anthracene, and phenanthrene. Chlorbenzene thus treated gave diphenyl, parachlordiphenyl, paradichlordiphenyl and an isomer of it, and diphenylbenzene.—*Liebig's Annalen*, clxxxix, 129, 135, October, 1877. G. F. B.

5. *Boracic Acid.*—In the Annales de Chemie et de Physique, for November, M. L. **Dieulafait** publishes a very interesting discussion as to the origin of the boracic acid found in the Suffoni of Tuscany, and in connection with many saline deposits, especially those of Stassfurt. He traces the boracic acid in all cases to what he calls the normal sea water, and he opens his paper with the following broad generalization: "Toutes les substances salines existant en amas et en couches dans les terrains sédimentaires ont fait primitivement partie d'une mer normale. D'un autre côte, à toutes les époques, les mers ont eu une composition qui ne s'éloignait pas sensiblement de celle des mers de la periode actuelle." In order to establish this conclusion in the case of the borates he gives in the first place experimental evidence that the water of the Mediterranean contains at least two decigrams of boracic acid in each cubic meter, and, further, that on evaporating the brine boracic acid accumulates in the bittern until after the deposition of carnallite. In the second place, he insists that in the very characteristic deposits of Stassfurt the borates are found above the carnallite as we should expect, if these deposits were formed

as assumed, by the drying up of extensive salt lakes. Again, having confirmed the previous statements that the chief salt beds of the world are found on two geological horizons, the Lias and the middle Tertiary, he gives evidence that in the Maremma of Tuscany where the Suffoni occur, there is a saliferous basin of the Tertiary period, and he concludes that the Suffoni are not properly volcanic vents, but that the surface water percolating to the salt beds—heated it is true by volcanic agency—determines well-known chemical changes, from which result the peculiar acid vapors there discharged. But we can only give here the barest outlines of an argument, which is worthy of careful study.

We might question whether the very broad generalization of the author could be fully sustained, and think that he underrates the effect of local influences in varying the composition of different saline beds; yet he seems to us to have correctly interpreted the general order of the causes which have produced the deposits of borates, and in the experimental results cited above, to have added two very important facts towards the solution of this interesting problem of chemical geology.

M. Dieulafait also contributes in his paper some important facts in regard to the certainty and delicacy of three principal tests for boracic acid. He rejects the test with turmeric as unsatisfactory, in the presence of such a mass of salts as are found in bittern, and he finds the flame reaction by far the most sensitive as well as the most trustworthy of all the tests with which he has experimented. When the Bunsen lamp is supplied with pure hydrogen, he finds that the flame reaction will indicate the one-millionth of a gram of boracic acid. His method of applying the test is as follows: The material to be tested is first mixed with an excess of oil of vitriol, and this paste held in a loop of platinum wire is brought near—say within four millimeters—but never nearer than two millimeters to the visible mantle of the hydrogen flame, so that the flame may not be colored in the least by the sodium always present. If the assay contains boracic acid, the characteristic green coloration appears, which can be identified with absolute certainty, by means of a spectroscope, and the coloration can be most delicately observed by looking through the mantle of the flame tangentially.

J. P. C., JR.

6. *Photo-electric Phenomena.*—R. BORNSTEIN continues his experiments on the influence of light on the electrical tension in metals, and shows that the effect is not a thermo-electric one. The photo-electric series of the metals runs in this order:—Aluminium, gold, copper, platinum, silver. While the thermo-electric series is as follows:—Silver, platinum, copper, gold, aluminium.

His conclusions are as follows:—

(1.) In a circuit consisting of two different metals, a photo-electric current is generated whenever the two junctions are exposed to luminous radiations of different intensities.

(2.) When the same junction is exposed in one case to an increase of temperature, and in another to a more intense illumination, the thermo-electric and photo-electric currents respectively generated in these cases are opposed to each other in direction.—*Phil. Mag.*, Nov., 1877, p. 330. J. T.

7. Hankel concludes from a series of experiments upon the photo-electricity of varieties of fluor-spar, that the electrical phenomena are largely due to the influence of the chemical rays of the spectrum, which cause chemical changes in the constitution of the crystal.—*Ann. der Physik und Chemie*, No. 9, 1877, p. 66. J. T.

8. *Specific Heat of air at constant pressure and constant volume.*—H. Kayser has redetermined the specific heat of air at constant volume and constant pressure, by a modification of Kundt's method. The dust figures in the glass cylinder were formed by the transverse vibrations of steel rods excited by a violincello bow. A small cork piston attached to one end of the rod played in the glass cylinder, while a style affixed to the other end of the rod drew its vibrations on a phonautograph. These vibrations were compared with those of a tuning fork. H. Kayser concludes from his experiments that the true value of the velocity of sound in free air is 332·5 m. The value of k, the specific heat, has been variously assigned as will be seen from the following table:—

Masson	1·419	Cazin	1·41
Weissbach	1·4025	Röutger	1·405

H. Kayser concludes that the true value is $k=1·4106$.—*Pogg. Ann.*, No. 10, 1877, p. 218. J. T.

9. *Fluorescence of the Retina.*—M. von Bezold and Dr. Engelhardt are led to the conclusion that the living retina also fluoresces under the influence of the same rays, which Helmholtz has stated exercise a fluorescent action on the dead retina.—*Phil. Mag.*, Nov., 1877, p. 397; *Trans. from Berichte d. baier. Akad. Math. Phys.*, June, July 7, 1877. J. T.

II. GEOLOGY AND MINERALOGY.

1. *Reports of the United States Geographical Surveys west of the One-hundredth meridian*, in charge of First Lieutenant Geo. M. Wheeler, Corps of Engineers U. S. Army, under the direction of Brig.-Gen. A. A. Humphreys, Chief of Engineers, U. S. Army. Vol. IV. Paleontology; quarto with 83 plates. Washington, 1877. Engineer department, U. S. Army.—This large volume, a contribution to the science of the country from the Wheeler expedition, under the War Department, comprises two important memoirs, as follows:

(1.) Report upon the Invertebrate Fossils collected in portions of Nevada, Utah, Colorado, New Mexico and Arizona, by parties of the Expeditions of 1871, 1872, 1873 and 1874; by Charles A. White, M.D.: comprising general observations upon the collections and the periods they represent; a general view of the classification adopted; and descriptions, in successive chapters, of the fossils of the Primordial, Canadian, Trenton, Subcarboniferous, Carboniferous, Jurassic, Cretaceous and Tertiary Periods; and illustrated by twenty-one plates.

(2.) Report upon the extinct Vertebrata obtained in New Mexico by parties of the Expedition of 1874; by Prof. E. D. COPE: comprising—(1) fossils of the Mesozoic periods, and geology of Mesozoic and Tertiary beds; (2) fossils of the Eocene; (3) fossils of the Loup Fork group; and illustrated by sixty-two plates.

Prof. White's valuable report has already been briefly noticed in vol. xii of this Journal (1876).

Prof. Cope's report embraces descriptions of a large number of vertebrate fossils, including species of fishes, birds, reptiles and mammals. Some of the general results arrived at with regard to the species of the Eocene of New Mexico are presented in vol. xii of this Journal, (1876, p. 297). The Loup Fork (or Loup River) group, as Dr. Cope observes, has now been identified at three widely separated localities: by Dr. Hayden in the Upper Missouri region, and by Dr. Cope in Colorado, and in New Mexico, the Santa Fé marls, first studied by Dr. Hayden, being of this horizon. The group underlies the "White River group" in the Missouri region, and has been regarded as Pliocene. Dr. Cope has described thirty-four species of Vertebrates from these beds, and points out the fact that while differing mostly in genera, and throughout in species, as far as now known, from those of the White River beds, they appear to be somewhat older in their geological relations than Pliocene, and hence, he has suggested (first in 1875) that they may be Upper Miocene. The genera favoring this supposition are stated to be *Amphicyon*, *Dicrocerus*, *Hippotherium*, *Aceratherium* (*Aphelops*), a *Mastodon* of the type of *M. angustidens*, *Pseudælurus*, *Steneofiber*. The species of the White River and Loup Fork groups differ widely in genera from those of the Eocene. The 62 lithographic plates of fossils illustrating Prof. Cope's Memoir are crowded with good figures. Nineteen of them are occupied with figures of parts of skeletons of different species of Coryphodon (Bathmodon of Cope 1872–1875) named by Dr. Cope, *C. cuspidatus*, *C. lobatus*, *C. obliquus*, *C. radians*, *C. latidens*, *C. elephantopus*, *C. molestus*, *C. simus*. Prof. Cope discusses several controverted points, which we leave without notice.

2. *Summary of field work of the United States Geological and Geographical Survey of the Territories*, under the charge of Dr. F. V. HAYDEN, for the season of 1877.—The work of the United States Geological and Geographical Survey of the Territories in charge of Dr. F. V. Hayden has been prosecuted with large success during the past year. The surveys in Colorado having been completed during the previous year, the parties prosecuted their work in a belt of country lying mainly in the western half of Wyoming, but also embracing adjacent portions of Utah and Idaho; all lying immediately north of the region embraced in the Survey of the 40th Parallel by Clarence King. The parties all took the field on the first of June.

The primary-triangulation party, upon the work of which that of all the topographical parties is based, was, as usual, in charge of Mr. A. D. Wilson, Chief Topographer. He took the field at Rawlins Springs, Wyoming. Near this point a base-line was carefully measured, from which a net-work of triangles was extended over the country from Fort Steel in Wyoming, westward to Ogden in Utah, a distance of about 260 miles, and north as far as the Grand Teton near the Yellowstone National Park. The area embraces about 28,000 square miles, and within it twenty-six primary stations were occupied and carefully computed, and, besides, many mountain peaks were located.* From the base-line at Rawlins, this work was carried north and west to the valley of Bear River in Idaho, where a check base was measured and the system expanded to the neighboring mountain peaks. Many remarkably long sights were taken from the loftier summits, several over 100 miles in length, and some of them 135 miles. From the Wind River Peak all the prominent points in the Big Horn Mountains to the northwest, were sighted, and the higher of the Uinta Mountains to the southwest, each about 165 miles distant. From these, connections were made at six points with the triangulation of the Survey of the 40th parallel.

In addition there were three other fully equipped divisions for topographical and geological work, and another under the direction of Dr. C. A. White, for special geological and paleontological work.

The area assigned to the Green River division, directed by Mr. Henry Gannett, with Dr. A. C. Peale, as geologist, was rectangle No. 56; the boundaries of which are meridians 109° 30′ and 112°, and parallels 41° 45′ and 43°. This area contains about 11,000 square miles, and embraces portions of Wyoming, Utah and Idaho. The party first surveyed that portion of the district which is drained by John Day's and Salt Rivers, two tributaries of the Snake. Thence, working southward and westward, they surveyed that portion of the district which is drained by Bear River.

With the exception of a small area of granite along the southwestern side of Wind River Mountains, and some basaltic outflows in the northwestern portion of the district, which are not older than late Tertiary age, Dr. Peale finds all the rocks to be of sedimentary origin. A comparatively small space is occupied by strata of Silurian age; the others range from Carboniferous to late Tertiary. A very large part of the district is occupied by beds of the Green River and Bridger groups, probably of Eocene age. Coal was found to exist in large quantity on Upper Bear River and its tributaries, and also on some of the branches of Green River, being especially abundant between Twin Creek and Ham's Fork. In the Malade valley, Dr. Peale observed deposits that are of later age than the Bridger group, but still, probably of Tertiary age.

The Sweetwater division in charge of Mr. G. B. Chittenden, with Dr. F. M. Endlich as geologist, covers rectangle No. 57, which is bounded by meridians 107° and 109° 30′, and parallels 41° 45′ and 43°, embracing about 10,800 square miles. The party took the field at Salt Wells station, on the Union Pacific Railroad, and worked northward and eastward. Owing to the advancing season, and the probable risk, on account of hostile Indians, about 800 square miles in the northeastern portion of the district were left unworked, and the party left the field on the 25th of September, near Fort Steele. Mr. Chittenden estimates that about five-eighths of the area surveyed are desert land; two-eighths mountainous, and one-eighth, of available value for habitation. A very large portion of the area is without the strong topographical features common to the surrounding region. Dr. Endlich reports that a very large part of the area of the district is occupied by strata of Tertiary age; that they found no older rocks than these after leaving the southern boundary, as they pushed their work northward, until they reached Fort Stambaugh. Here the oldest metamorphic rocks are exposed, with the Tertiary strata resting upon them. Going eastward from Fort Stambaugh upon the Wind River drainage, he found the full series of the sedimentary formations represented, beginning with the Silurian. Here, as well as in the mountains farther west, he reports abundant evidence of the former existence of glaciers. Marching southward the older sedimentary formations were found in the Seminole hills. Northward from Rawlins he reports the existence of mud-puff springs, covering an area of about two square miles, similar to the famous mud-geysers, but on a small scale.

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* A map showing the primary triangulations of this survey has quite recently been published.

The party assigned to the Teton division was in charge of Mr. G. R. Bechler, with Professor O. H. St. John as geologist. This division lies between meridians 109° and 112°, and parallels 43° and 44° 15′, and is drained by the upper branches of the Shoshone or Snake River. The district almost throughout is a mountainous one, the mountains presenting several comparatively short but distinct ranges, among which are the Mount Putnam, Blackfoot and Caribou Ranges; many peaks of which are largely covered with snow during the whole summer. The party left the field prematurely on account of the probable danger from hostile Indians, but not until after surveying 6,000 square miles.

Professor St. John reports the district to be one of unusual interest, and worthy of much and careful study. With the exception of the rocks of igneous origin, which he finds to have been of late Tertiary outflow, and quite extensive, the region is occupied by sedimentary or stratified rocks; which he refers to Lower Silurian, Carboniferous, Jura-Triassic, Cretaceous and Tertiary ages. They have all suffered great displacements in nearly all parts of the district. Besides these, he mentions other deposits in the valley of Snake River, later than the Tertiary strata just referred to, but still probably of Tertiary origin. It seems probable that these, as well as similar deposits observed by Dr. Peale, may prove to be of the same age as the Lake Beds of Dr. Hayden in Middle Park, the Uinta Group of Mr. King, &c.

The necessity of a careful examination of the various geological formations in the field, and of a review by a practical paleontologist of the various districts that have from year to year been surveyed by the different geologists of this and other surveys, has been long felt. Such a work indeed, was imperatively necessary before a consistent and comprehensive classification of the formations could be established. This duty was assigned to Dr. C. A. White, the paleontologist of this survey, and he took the field at the beginning of the past season, continuing his labors until its close. He has pursued his researches with such success as to demonstrate the necessity of continuing this class of investigations by various lines of travel across what is generally known as the great Rocky Mountain region, especially those portions of it that have been surveyed, as well as those in which the surveys are now in progress. Spending some time in the plains of Colorado immediately east of the Rocky Mountains, he crossed the mountains by way of Boulder Pass and Middle Park to the region of Yampa and White Rivers; thence, crossing Green River, he pushed his investigations westward along the southern base of the Uinta chain, as far as Great Salt Lake. Thence recrossing the Wasatch Mountains, he carried his work eastward across the Green River basin.

Among other important results, he has demonstrated the identity of the Lignitic series of strata east of the Rocky Mountains in Colorado with the Fort Union group of the Upper Missouri River, and also its identity with the great Laramie group of the Green River Basin and other portions of the region west of the Rocky Mountains; to which the labors of Dr. Hayden and Mr. Meek have long since plainly pointed.

He also finds the planes of demarkation between any of the Mesozoic and Cenozoic groups, from the Dakota, to the Bridger inclusive, to be either very obscure or indefinable, in the region traversed by him; indicating that whatever catastrophal or secular changes took place elsewhere during all that time, or extending within the limits of that region, sedimentation was probably continuous within what is now that part of the continent, from the earliest to the latest of the epochs just named. During the progress of the field work, large and very important collections of fossils were made, which are now being investigated.

Messrs. S. H. Scudder of Cambridge and F. C. Bowditch of Boston spent two months in Colorado, Wyoming and Utah, in making collections and observations in fossil and recent entomology, with very gratifying results. Mr. Scudder is making arrangements to add materially to his labors in this department, in connection with the survey.

Professor Joseph Leidy spent some time during the season in Green River Basin, making observations and collections for his large work on Rhizopoda, which is to form one of the quarto volumes of the survey.

The botany of the survey was represented during the past season by the two great masters, Sir Joseph D. Hooker, Director of the celebrated gardens at Kew, England, and Professor Asa Gray of Harvard University. Their examinations extended over considerable portions of Colorado, Wyoming, Utah, Nevada, and California; but their own personal observations in the field will by no means be a measure of the extended range of the subject that will be covered by their report. They have for years been watching and noting the progress of the work of American botanists, and are ready to bring out some most important generalizations in their report. Dr. Hooker has, during many years past, prosecuted extensive botanical researches, not only in Europe, but in the Indies, from the Bay of Bengal across the Himalayas to Thibet; in the Antarctic regions, the southern parts of South America, and Africa; in New Zealand, Australia, Morocco, and Asia Minor. The joint report of these two savants will form a part of the eleventh annual report of the survey; and as it will embrace facts and views of world-wide comprehensiveness, it cannot fail to be of great scientific interest.

Previous to the year 1874 there were many indefinite rumors of the existence of strange and interesting remains of the habitations and implements, especially pottery, of a long-departed and forgotten people, who once occupied the region about the head-waters of the San Juan. The various government exploring parties that have traversed New Mexico and Arizona brought reports of other ruins in those regions, but no reliable accounts of those just referred to had ever been received.

In 1874, Mr. W. H. Jackson, in charge of the Photographic department, was directed to visit and report upon those ruins, in connection with his usual work; which he did, and the results were published in the Bulletin of the Survey. In the following year, Mr. W. H. Holmes, one of the geologists of the Survey, visited the same region and made a careful investigation of the ruins, and by his artistic skill, in connection with Mr. Jackson, brought back complete illustrations, as well as a full report. Upon his return Mr. Holmes conceived and successfully carried out the plan of representing these ruins by models in plaster. Mr. Jackson has taken up, and added to his department, the work of reproducing these models as well as those of ancient pottery found with them. In furtherance of this work he visited Northern New Mexico and Arizona, during the early part of the past season. He procured, among other important material, the necessary data for plastic representation of the pueblos or communal town-dwellings, of Taos and Acorna, models of which he has already constructed. Contact with Europeans has somewhat modified their ancient style of building, but one can readily see that they are constructed after their ancient prototypes, the dwellings of the forgotten people; forgotten, because the builders of the modern structures are as ignorant of the ancient builders as we are ourselves.

These are merely the salient features of the work which has been accomplished by the survey; the details will appear in its usual publications. H.

3. *Geology of Wisconsin, Survey of* 1873–1877. Vol. II, 768 pp. royal octavo, illustrated by several wood-cuts, lithographs and maps in the text, and accompanied by an atlas of maps.—The legislature of Wisconsin passed its first act for a geological survey in 1853, appointing Mr. E. Daniels, State-geologist; and its second in 1854, putting J. G. Percival in this position. Two carefully prepared Reports, of about 100 pages each, by Dr. Percival were published in 1856. In 1857, the survey was reorganized under Prof. James Hall, with whom was associated in 1859, Prof. J. D. Whitney with special reference to a study of the lead region. Dr. Carr and Mr. Daniels were engaged with Prof. Hall during the year 1858. A Report of this survey by Professors Hall and Whitney, making an octavo volume of 456 pages, was published in 1862. The volume is one of great value and especially on account of the descriptions of the mines, ores and minerals, by Prof. Whitney, which occupy 352 of its pages. A second volume was prepared by Prof. Hall, but it was not published. In 1873 a new survey was ordered by the State and placed under the charge of Dr. I. A. Lapham. Dr. Lapham held this position for two years, and annual reports were prepared and presented to the legislature, the publication of which the legislature failed to order. Dr. Lapham was displaced, unreasonably, in 1875, and the place given to Dr. O. W. White. But in February, 1876, it again passed into the hands of able geologists through the substitution of Prof. T. C. Chamberlain, of Beloit College, and under this arrangement, the new volume, above announced, has been prepared and issued.

This volume (called vol. II, that of Professors Hall and Whitney being vol. I) contains Dr. Lapham's annual reports for 1873, 1874, and Dr. White's for 1875 as introductory to the reports of the recent survey. These final Reports are:

I On the Geology of Eastern Wisconsin, by Prof. Chamberlain, 310 pages.

II. On the Geology of Western Wisconsin, by Prof. Roland D. Irving, 235 pages.

III. On the Geology and Topography of the lead region, by Prof. Moses Strong, 110 pages.

These Reports show that the work has been carried forward with ability and care, and with a full appreciation of what both science and the economical interests of the State demand. The topography, hydrology, forest and marsh vegetation and soils, are the subjects of valuable chapters; and then the distribution and characteristics of the several rock-formations are given with full details. Under the subject of erosion many remarkable facts are stated; and the frontispiece represents a lofty tower or "Stand" Rock, of Potsdam Sandstone, which is almost as remarkable as anything of the kind in Rocky Mountain scenery. The chapters on the Drift and other Quaternary deposits are of unusual interest. The third report, on the lead region, is, as its author states, much briefer than the subject demands. It serves to supplement and extend the account by Prof. Whitney, adding the results afforded by the more recent mining operations and a further study of the regions.

The Atlas consists of a series of colored plates, illustrating in sections the geology of the State; and others for eastern Wisconsin, representing by color the distribution of the native vegetation, the drift, and the various kinds of subsoils. The chromolithographic work, which is beautifully done, is credited to the Milwaukee Lithographic and Engraving Company. The size of the most of the maps is inconveniently large—too large, we think, for the small amount of detail in the geology.

We cite from the volume the following facts and conclusions relating to the Wisconsin drift.

In the first place the facts with regard to the *driftless region*, which covers Southwestern Wisconsin and the borders of Minnesota and Iowa adjoining, described and mapped by Prof. J. D. Whitney, are brought out with additional observations; and the view of Dr. Percival, its first describer, and Prof. Whitney is sustained—that it has no bowlders or drift over its surface, and no water-worn material except in the beds of some of the streams. Its area is about 12,000 square miles.

The eastern border of the driftless area, as laid down by Prof. Irving, lies near a line running north from Freeport in Illinois to Grand Rapids. Thence, the boundary bends westward and northwestward near the line of the Green Bay and Minnesota railroad, and passes twelve miles north of Eau Claire. According to N. H. Winchell, it crosses the Mississippi just south of St. Paul, and then, to the south, enters Iowa over the southwest corner of the southeast town of Minnesota—that of Houston. The former absence of the ice is proved by the absence of gravel and stones, which suddenly cease on entering the region, and, as Prof. Irving states, by the character of the hills and ravines and the existence over it of numerous fragile sandstone peaks. The origin of this driftless feature of the region receives important elucidation from the facts observed by Professors Chamberlain and Irving, and the latter brings out, in his report, a new theory in explanation of it.

According to the observations of Prof. Irving, in connection with those of Prof. Chamberlain, the part of the great northern glacier that moved over Lake Superior was prolonged southwestward along several great depressions: (1) along the great Lake Michigan depression south-southwestward; (2) along the Green Bay Valley, which has a nearly parallel course (S. 35° W.) and extends nearly to Illinois; also, on the north, and more to the west (3) along a Keweenaw Bay depression, west of southwest in direction; (4) along a Bayfield Bay depression, as a part of, or a branch of, (5) the greater mass moving in the same direction from the western extremity of the Lake Superior depression. The Michigan Bay ice-mass stretched on south and west over Illinois. That of the Green Bay Valley was partly independent and hardly reached to Illinois. While those of the Keweenaw Bay depression and Western Lake Superior continued westward and southward over Minnesota, and thence, as N. H. Winchell has shown, south to Iowa, where it was connected with the ice over northern Illinois.

The independence of the glacier-mass of the Michigan Bay depression and that of the long Green Bay valley is well proved by Prof. Chamberlain. Between the lake and the valley there is a low ridge called the Kettle Range—so named from the multitudes of ponds and pools over its surface. It is one to ten miles wide, and in some parts 300 feet high above its base. It continues southward nearly to Illinois, here bends around westward, and then turns northward along the west side of the valley and near the boundary of the driftless area. This Kettle Range is, according to both geologists, a true moraine ridge, and marks the east, south and west outlines of the Green Bay valley glacier. It consists of gravel, bowlders, sand and clay, unstratified, but with portions here and there rudely stratified. Furthermore, as ascertained by Prof. Chamberlain (the geologist in charge of Eastern Wisconsin) the glacial scratches made by the Michigan and Green Bay ice-courses converge southward toward the axis of the intervening Kettle Range, having a southwest direction on the Michigan Bay side and a southeast on that of the Green River Valley, thus pointing to the range as a moraine ridge between the two ice-masses or along their blending borders. Again, on the west side of the Green Bay Valley the glacial scratches run southwestward (while southeastward on the east side) and terminate in the Kettle Range of that side, thus marking out this western branch of the Kettle Range to be the course of the western border and western moraine of the Green Bay glacier. These facts are well displayed by Prof. Chamberlain on plate VII. The pond-depressions, or bowls and basins, of the Kettle Range are accounted for by this geologist on the supposition of alternate retreats and advances in the glacier, producing irregularities in the deposition of ridges of moraine material. He says also that there are numerous subterranean streams running from its base which undoubtedly have an undermining action and may have produced part of the depressions. Over the regions of Wisconsin between the Green Bay glacier and the more northern Keweenaw Bay glacier course the ice thinned out toward the great driftless area.

Professor Irving accounts for the absence of ice from the driftless area on the view that the great glacier of the north was divided by the deep and large channels of Michigan Bay and Lake Superior; that the great depth of the ice of Lake Superior forced it to follow the courses of the bay-depressions leading out of it, one part going south-southwestward along Michigan Lake and the Green Bay valley, and the rest west of southwest along the Lake Superior bays; and that thus the intermediate region was left iceless and driftless. He states that the surface is not higher than that of Wisconsin to the east, and is lower than that of Minnesota to the west; and hence that no argument can be drawn in favor of its escape from the ice by its altitude or by an elevation of the land. The explanation, though different, is closely related to that given by Professor N. H. Winchell, in his Minnesota Geological Report for 1876, (published in 1877)* and that the hills of the granitic region stretching southwestward from Keweenaw Point, and from the south shore of Lake Superior farther west, prevented the extension of the great glacier from Lake Superior in that direction.

Both Professor Chamberlain and Professor Irving state that there is abundant evidence that during the Glacial era the continent in that part was higher above the sea-level than now; and that this elevation was followed by a depression below the present level —that of the Champlain period. The former states that "some of the streams have cut channels from one to three hundred feet deeper than those they now occupy," thus pointing to the fact of greater elevation during the Glacial era.

The chapters on the drift contain numerous facts with regard to the sources of the drift, proving transportation for 100 to 300 miles or more. The masses of native copper, which are very common, must have come, it is stated, from Keweenaw Point, or full 300 miles.† All the facts brought out on the glacial phenomena are of great significance and merit reproduction in this place; but we have to refer to the volume for the larger part of them. The observations on the Champlain deposits and terraces are also highly interesting.

Only two other facts we cite here. The Milwaukee brick have a cream-white color and this has been attributed to the absence of iron. But Professor Chamberlain states that the clay is *red*, and contains, according to analyses of the brick, nearly *five* per cent of oxide of iron; and that the absence of color must be due to the formation of a silicate of lime and iron, lime being also present in the clay. [The silicate is probably a variety of epidote, the formation of which in the Triassic red sandstone of the Connecticut valley where it adjoins trap dikes has often been observed by the writer to be connected with a discharge of the red color of the sandstone.]

The Niagara limestone of southern Wisconsin includes two distinct varieties of limestone which were of simultaneous origin; and, according to Professor Chamberlain, the compact kind corresponds to the solid coral limestone of modern coral reef seas, and the granular to the beach sand-rock, which is simultaneously made along the shores of the coral reef regions out of coral sands.

J. D. D.

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* Noticed briefly in the number of this Journal for November last, vol. xiv, p. 422.

† Mr. A. H. Worthen states, in the first volume of his Report on the Geology of Illinois, that in the Mississippi bluffs at the mouth of Fevre River, a few miles below Galena (within the driftless area but not far north of its southern limits) he has observed small boulders of trap and porphyry, and Mr. A. H. Beebe informed him of the discovery of native copper at the same place. Prof. C. A. White, in his Report on the Geology of Iowa, states that a mass of native copper was found in Lucas county, near the middle of southern Iowa, which weighed more than thirty pounds. Galena is about 350 miles in a straight line south southwest from the Keweenaw copper region, and Lucas county is 170 miles southwest-by-west from Galena, or about 465 miles southwest-by-south from the Keweenaw region.

4. *Probable ancient outlet of the Great Salt Lake.*—It is believed that the explorations of the survey under the direction of Dr. Hayden, the past season, have determined the probable ancient outlet of the great lake that once filled the Salt Lake Basin. At the head of Marsh Creek, which occupies the valley, continuing directly south from that of the lowest Portneuf, is the lowest pass between the Great Basin and the drainage of the Columbia. In fact so low and flat is it, that a marsh directly connects the two streams, one flowing to the Bear River and the other to the Portneuf and Snake Rivers.

This fact was observed by the Survey in 1871 and 1872, but this district has been carefully examined the past season by Mr. Gannett and Dr. Peale.

5. *Siberian Steppes.*—Professor John Milne, in a paper entitled "Across Europe and Asia, Part V, from Ekaterinburg to Tomsk" (Geol. Mag., Oct., 1877), suggests that the material of the great plains of Siberia was deposited by the rivers while they were under floods caused by their being dammed about their mouths in consequence of the ice of the stream not having there melted. He shows that although the time of freezing of the waters in autumn differs but a week or so in the more northern and southern parts of the rivers, the time of melting in the spring often differs a month. Consequently the ice toward the mouth of the stream might serve as a dam during the breaking up of the cold season, and cause a spreading of the waters over the country; and "in past times, when the cold was probably more intense, these barriers of ice may have been more continuous and complete, and thus have kept the plains—which were then smaller than they are at present, because their northern ends were beneath the sea—more or less constantly covered with a lake of turbid water." Floods from this source occur now in Siberia. This barrier of ice differs from that suggested by Mr. Belt, in being of fluvial instead of Arctic-ocean origin. Professor Milne remarks that the Siberian plains are directly connected with the several river channels. Wherever there is a river, and especially a large one, a broad plain accompanies it; and as it expands in flowing northward, so with the plain. The widening of these plains continues until they unite to form that open flat expanse which fringes the Arctic Ocean. In some parts, the plains 1,000 miles inland are not over 250 feet above the sea-level.

6. *Mikroskopische Physiographie der massigen Gesteine*, von H. ROSENBUSCH. 596 pp. 8vo. Stuttgart, 1877. (E. Schweigerbart'sche Verlagshandlung.)—The present work forms properly a second companion volume to the *Mikroskopische Physiographie der petrographisch wichtigen Mineralien*, published by Dr. Rosenbusch in 1873. The many lithologists who have used with pleasure and profit his earlier work, will welcome the one which has just been given to the public. Like its predecessor, it is not a compilation of facts observed by others, but rather a record of the author's own wide and extensive observations. The general

method of classification adopted is as follows:—(1) orthoclase rocks; (2) orthoclase-nepheline, and orthoclase-leucite rocks; (3) plagioclase rocks; (4) plagioclase-nepheline, and plagioclase-leucite rocks; (5) nepheline rocks; (6) leucite rocks; (7) olivine, or chrysolite rocks. The special system of subdivisions of these classes will be gathered from the following example: Orthoclase rocks are divided into those which are pretertiary or *older* rocks, and those that are tertiary or recent, or *younger* rocks. First, the older rocks (I) contain quartz and are (1) *granular* in texture, granites, or (2) *porphyritic*, quartz porphyries, or (3) *glassy*, felsite-pitchstones; or they are (II) without quartz, and are (1) *granular*, syenites, or (2) *porphyritic*, porphyries with no quartz. Secondly, the younger rocks (I) contain quartz, and are (1) *granular* or *porphyritic*, liparites, or (2) *glassy*, obsidian, trachytic pitchstone, perlite, pumice; or they are (II) without quartz, including the trachytes and some glassy rocks.

The description of the individual rocks are very complete, especially the references to their microscopic character. A valuable portion of the work is the list of books and memoirs on lithological subjects covering about thirty pages. E. S. D.

7. *A Guide to the Determination of Rocks*, being an introduction to Lithology; by E. JANNETAZ. Translated from the French by G. W. Plympton, C.E., A.M. 165 pp. 8vo. New York, 1877. (D. Van Nostrand.)—This little book has some very good points, but its usefulness is much impaired by the numerous inexcusable mistakes which appear in its pages:—for example, *muscovite* micas are stated to be rich in magnesia; the tesselated appearance of some andalusite crystals (chiastolite or macle) is described as due to the "envelopment of fragments of the rock in which they are found," (p. 36); pyrites (FeS_2) is said to be easily reduced to FeS (p. 109) etc. Besides, such words as Cordieritfels, Cyanitfels, Topfstein, Gallinace, etc., do not belong to the *English* language.

8. *Tables for the Determination of Minerals.* Based upon the tables of Weisbach; enlarged, and furnished with a set of mineral formulas, a column of specific gravities and some of the characteristic blowpipe reactions; by PERSIFOR FRAZER, Jr., A.M. 119 pp. 8vo. Philadelphia, 1878. (J. B. Lippincott & Co.)—The first edition of Professor Frazer's useful little book was noticed in volume ix of this Journal. The revised work, besides numerous corrections and minor additions, contains some new features, as for instance, the indication of the comparative rarity of the less prominent species.

9. *Tridymite in Ireland.*—Prof. A. von Lasaulx reports the discovery of tridymite in the trachyte-porphyry of County Antrim, Ireland.

10. *Anthracite of Pennsylvania.*—Mr. E. T. Hardman, in a paper in the Journal of the Royal Geological Society of Ireland (xiv, 200, 1876), attributes the change to anthracite in Pennsylvania to the series of trap-dikes to the eastward—not recognizing the fact that these trap-dikes are Triassic or Jurassic in age, and the nearest over fifteen miles distant from the coal.

III. BOTANY AND ZOOLOGY.

1. C. DARWIN. *The Different Forms of Flowers on Plants of the me Species.* (London, Murray; New York, D. Appleton & Co. 77.) 12mo, 352 pp.—Circumstances have prevented an earlier tice of this volume, Mr. Darwin's last work upon the fertilization flowers, the English edition of which was issued last summer, d the American reprint was not far behind. Although we ought call attention to it, for the benefit of our general readers, and of e numerous local botanists of our country who have little access foreign publications, yet the duty of reviewing the present vol- ae was not urgent, as regards scientific novelty. For it is to a eat extent a reprint, with alterations and considerable additions, articles published some years ago in the Journal of the Linnæan ciety, which excited much interest at the time, and the topics ve become a part of our common knowledge. Still Mr. Darwin uld not take up and reprint these papers without adding some- ing to their value, and without making emendations or indica- ig qualifications. He adds, moreover, succinct notices of what s been done by others in the same field.

Six of the chapters relate to dimorphous blossoms, such as those Primrose and *Houstonia*, including also the trimorphic cases, as *Lythrum Salicaria* and some species of *Oxalis*. The seventh apter discusses Polygamous, Diœcious, and Gyno-Diœcious ants; the eighth and closing chapter is devoted to Cleisto- mous Flowers.

For the dimorphous and trimorphous forms,—which needed general appellation, and one to indicate the difference in the xual organs themselves (calyx, corolla, etc., being alike in the o sorts),—Mr. Darwin adopts Hildebrand's term of *heterostyled*. hen this term came to our notice as one intended for settled use, e took the opportunity in this Journal, a year ago, to suggest a ter name, one which equally avoids the ambiguity of the older rm, *dimorphous*, by indicating that the difference is in the stamens id pistils, not in the floral envelopes, and avoids the erroneous iplication of the term *heterostyled*, that the style is only or mainly ncerned. That is, we proposed the term *heterogone* or *hetero- mous*. We were too late to ensure its adoption in this work. A irly good term once in use ought not to be exchanged for a new ie without very sufficient reason; and for the present purpose the rm *heterostyle* is well enough. But the time has arrived when is peculiarity of structure must be indicated in descriptive otany as a part of the character of the genera or species which fect it: and here the inconvenience or equivocation of the phrase *lores heterostyli* will sometimes be manifest. We think it proba- e that our term may find its place in systematic botany, and that e shall write *Flores hermaphroditi*, *heterogoni*, *monœcei*, *diœcei*, *no-diœcei*, *polygami*, as the case may be.

One good set of terms for phytography we owe to Mr. Darwin id the present book, i. e., that of *gyno-diœcious* and *gyno-monœ-*

cious, for the case of those plants which produce their two kinds of blossoms as hermaphrodites and females, either on distinct individuals or on the same plant. So, likewise, the term *andro-monœcious* and *andro-diœcious* for the case of hermaphrodite and male flowers, on the same or on separate individuals. As to andro-diœcism, Mr. Darwin remarks that, after making enquiries from several botanists, I can hear of no such cases. The last summer brought one such case to light in our Cambridge Botanic Garden, perhaps exceptionally, but it raises the inquiry whether *Diospyrus Virginiana*, our Persimmon tree, may not be of this character. A solitary female tree here, and with no male tree in the town, sets fruit more or less in most seasons; but the persimmons are undersized and seedless. This year it was loaded with full-sized fruit, well furnished with seeds, the latter with a good embryo. The female flowers always bear stamens; but these are generally thought to be impotent; perhaps they usually produce some pollen; they doubtless did so upon this occasion.

As Mr. Darwin asserts, it would be convenient, and conduce to clearness, to restrict the Linnæan (and as now used loose) term *polygamous* to the species in which hermaphrodites, males, and females co-exist. This may occur in two ways, and possibly in three. The English Ash, as he remarks, is triœcious, or has the three kinds on as many individual trees; while some Maples bear all three on the same tree.

If we rightly read a statement on p.10, it implies that proterandry and proterogyny are known to occur only in "some few hermaphrodite plants." But it can hardly mean that, cases of it being common and obvious in many natural orders.

The first chapter of this volume is devoted to *Primula* and its allies; the second, to hybrid Primulas, mainly to the Oxlip, which is shown to be a spontaneous hybrid between the Cowslip and the Primrose. A note is added on some wild hybrid Verbascums, specially those between *Verbascum Thapsus* and *V. Lychnitis*, which cross with the greatest facility, and produce a series of forms which almost connect these two widely distinct species, yet the hybrids of the first generation are almost wholly self-sterile. Such cases as this and that of the Oxlip, which was formerly thought to prove that the Cowslip and the Primrose were mere varieties of one species, show, as Mr. Darwin remarks, "that botanists ought to be cautious in inferring the specific identity of two forms from the presence of intermediate gradations; nor would it be easy in the many cases in which hybrids are moderately fertile, to detect a slight degree of sterility in such plants growing in a state of nature and liable to be fertilized by either of the parent species."

The third chapter takes up in succession other heterogone dimorphous flowers, particularly those of some species of Flax, and of *Houstonia*, *Mitchella*, and other *Rubiaceæ*. The fourth chapter discusses the trimorphous flowers of the same category, notably of *Lythrum Salicaria*, of which we gave an abstract when this striking case was first brought to light. Our *Nesæa verticillata* is also referred to, the trimorphous species of Oxalis considered, and finally *Pontederia*, the only monocotyledonous genus now known to be heterogone. The trimorphism in this genus was detected a few years ago by Fritz Müller in Brazil; also recently, in *P. cordata*, our common Pickerel-weed, by Mr. Leggett of New York. Chapter VI is a detailed discussion of experiments on the illegitimate offspring of heterogone flowers; i. e., offspring produced by breeding within the limits of the same form, short-styled with long-stamened, or the converse. The conclusion is that in all points "the parallelism is wonderfully close between the effects of illegitimate and hybrid fertilization. It is hardly an exaggeration to assert that seedlings from an illegitimately fertilized heterostyled plant are hybrids formed within the limits of one and the same species. This conclusion is important; for we thus learn that the difficulty in sexually uniting two organic forms, and the sterility of their offspring, afford no sure criterion of so-called specific distinctness. "If one were to cross two varieties of the same form of *Lythrum* or *Primula* for the sake of ascertaining whether they were specifically distinct, and he found that they could be united only with some difficulty, that their offspring were extremely sterile, and that the parents and their offspring resembled in a whole series of relations crossed species and their hybrid offspring, he might maintain that his varieties had been proved to be good and true species; but he would be completely deceived." The cause of this sterility between individuals which may have sprung from the very same parent or parents and from the same capsule, must evidently be in their reproductive organs only, and in some recondite incompatibility of their sexual elements, not in any general difference of structure or constitution. And Mr. Darwin effectively argues that the same holds in case of distinct species of the same genus. "We are indeed led to this same conclusion," he adds, "by the impossibility of detecting any differences sufficient to account for certain species crossing with the greatest ease, whilst other closely allied species cannot be crossed, or can be crossed only with extreme difficulty. We are led to this conclusion still more forcibly by considering the great difference which often exists in the facility of crossing reciprocally the same two species; for it is manifest in this case that the result must depend on the nature of the sexual elements, the male element of the one species acting freely on the female element of the other, but not so in a reversed direction." Sterility of hybrids ceases to be a criterion of species.

The 6th chapter follows up the subject in a series of concluding remarks. It refers to those cases of more or less marked reciprocal differences in stamens and style which are unaccompanied by any difference in size or form of pollen-grains; and it tabulates the difference in pollen-grains of the two sorts. "With all the species in which the grains differ in diameter, there is no exception to the rule, that those from the anthers of the short-styled form, the tubes of which have to penetrate the longer pistil of the long-styled form, are larger than the grains from the other form." "This curious relation led Delpino (as it formerly did me) to believe that the larger size of the grains is connected with the greater supply of matter needed for the development of their longer tubes." But it proved that, in many cases where the pollens differ much in size, the styles differ moderately in length, and *vice versa*, and that in plants generally, there is no close relationship between size of pollen and length of style (the grains being of the same size in *Datura arborea* and in Buckwheat, while the style of the one is nine inches long and of the other very short); yet still "it is difficult quite to give up the belief that the pollen grains from the longer stamens of heterostyled plants have become larger in order to allow of the development of longer tubes." A list of the genera, thirty-eight in number, positively known to be heterogonous is given. They belong to fourteen orders; but almost half the genera belong to the order *Rubiaceæ;* with the exception of *Pontederia*, they all have regular corollas, and all depend on insects for fertilization. "Plants which are already well adapted by the structure of their flowers for cross-fertilization by the aid of insects often possess an irregular corolla, which has been modeled in relation to their visits; and it would have been of little use to such plants to become heterostyled. We can thus understand why it is that not a single species is heterostyled in such great families as the *Leguminosæ*, *Labiatæ*, *Scrophulariaceæ*, *Orchideæ*, etc., all of which have irregular flowers."

Chapter VII relates to Polygamous, Diœcious, and Gyno-diœcious plants. A few genera are mentioned which have probably passed on from the heterogone condition to the diœcious. *Coprosma* is perhaps the best marked case; and *Mitchella* and *Epigæa* show tendencies in the same direction. On the other hand, Mr. Darwin's observations on *Euonymus Europæus* are "very interesting, as showing how an hermaphrodite plant may be converted into a diœcious one." *Rhamnus lanceolatus* shows the same thing more incipiently. Of Gyno-diœcious plants, which bear hermaphrodite and female flowers, but no separate males, and which show no obvious tendency towards diœcism, the principal illustrations are from *Labiatæ*, such as Thyme, *Nepeta Glechoma*, Mint, etc.

The eighth and last chapter is devoted to Cleistogamic flowers. All ordinary cases of two kinds of flowers are evidently arranged to favor or secure cross-fertilization. But there is a good number of plants, such as most Violets, which besides their ordinary and showy blossoms, produce others which fertilize and fructify without opening. These are always small and inconspicuous; and they so much resemble early flower-buds of arrested development that we were accustomed to designate them as flowers precociously fertilized in the bud. In some if not most cases this would be a quite correct representation of them; and there are well known instances in which—at least in cultivation—the earlier of the ordinary flowers self-fertilize without expanding or fully completing their development; but in others these comparatively minute and ever closed flowers are profoundly modified structurally in reference to their function. Dr. Kuhn, in 1867, gave them the appropriate name of *flores cleistogami*, *cleistogamic*, or as we prefer *cleistogamous* flowers. The literature of the subject may mostly be gathered from this chapter, in which all that is known of these blossoms is condensed. We cannot here attempt a recapitulation. In brief, "they are remarkable for their small size and from never opening, so that they resemble buds; their petals are rudimentary or quite aborted; their stamens are often reduced in number, with the anthers of very small size, containing few pollen-grains, which have remarkably thin transparent coats, and which generally emit their tubes while still enclosed within the anther-cells; and lastly the pistil is much reduced in size, with the stigma in some cases hardly at all developed. These flowers do not secrete nectar or emit any odor. . . . Consequently insects do not visit them; nor if they did could they find an entrance. Such flowers are therefore invariably self-fertilized; yet they produce abundance of seed." Indeed they are far more fertile than the ordinary flowers of the species, which are apt to be sterile. The latter are in most cases adapted to the visits of insects; in some, such as Orchids, they are dependent upon this agency for such fertility as they possess.

Cleistogamous flowers are known in about twenty-four natural orders, yet not in a large number of genera. The list given by Kuhn, and corrected and extended by Darwin, is likely to be enlarged; but in one particular it may be diminished, for *Ruellia*, *Dipteracanthus*, and *Cryphiacanthus* are really all of one genus. We can add another genus and natural order to the list. For, while writing this notice, Mr. C. G. Pringle, of Charlotte, Vermont, calls our attention to its occurrence in *Dalibarda repens*, of the order *Rosaceæ*, and sends excellent specimens which exemplify it. This should confirm the genus, which, as restricted to its original and proper species, and irrespective of this newly-discovered peculiarity, surely ought not to be combined with *Rubus*.

Cleistogamy is an arrangement to secure a certain and abundant supply of seeds with the least expenditure; it is a corrective of or guard against the dangers of cross-fertilization dependent on either winds or insects; but no cleistogamous species is known which has not ordinary flowers also, mostly corolliferous and insect-visited, some specially modified for such visits, either by heterogone dimorphism or by special structure such as that of Orchids and Violets, but some anemophilous, such as a few rushes and grasses. Among the latter, it is singular that one of the earliest known and strongly marked cases, that of *Amphicarpum* (*Milium amphicarpum* Pursh), should be overlooked.

Since this notice was written, Mr. Pringle has announced to us the discovery of cleistogamous flowers regularly occurring within the leaf-sheaths of *Danthonia spicata* and its allies, also in *Vilfa* and other grasses. A. G.

2. *Ferns of North America.* By DANIEL C. EATON, of Yale College. (S. E. Cassino, Salem, Mass.) Part I, pp. 20, and three plates, representing four species. 4to, 1877.—We are glad that our country is not to lag behind others in furnishing to amateurs as well as to serious and more general botanists the great facilities which figures give for the study of Ferns. Books of this kind, of various pretension and merit, abound in Great Britain, and there is evidently a great demand for them. But our own fern-fanciers are becoming numerous and active, and this work will aid them and bring many more into the field. At least our botanists and botanical students want it. And this work seems to us well planned to meet all these requisitions. Well executed it certainly is, thus far. The paper and typography are most excellent; and the plates, from colored drawings by J. H. Emerton, are beautiful specimens of chromo-lithography. Plate II, representing *Cheilanthes vestita* and *C. Cooperæ* (a new species, detected in California by Mrs. Elwood Cooper, whose name it bears), is to our mind the best; and the synopsis of the species of the genus, arranged under the sections, was a good thought. The figure of *Asplenium serratum*, a tropical American Fern, recently discovered in Florida by Dr. Garber, is well managed and characteristic; but by lamplight the green is too blue. Under the same conditions the *Lygodium*, which is well chosen for a leader, seems too pale and dull. A little more practice will set this all right. The prospectus informs us that a fascicle will be published every two months, or thereabout, at a dollar each; and that it is proposed to figure all the species indigenous to the United States. This is an excellent beginning. That it will be kept up to the standard, and will be critically accurate, we cannot doubt; for no one knows our Ferns half so well as Professor Eaton, and no one can describe them better. We hope, and we do not doubt, that the sale will warrant this enterprise, and ensure its continuance and completion.

A. G.

3. *Notes on Botrychium simplex*, by GEORGE E. DAVENPORT, 1877.—Here is more Fern-lore, an exhaustive monograph of a pygmy Fern, which was first described in this Journal (for 1823, vol. vi, in a page which is now reproduced in fac simile), and of a closely allied species *B. matricariæfolium*, from which it is now distinguished by the radical or rather subterranean sterile branch of the frond. Two large quarto plates crowded with figures (48 in number), illustrate the forms which these two species assume in this country, and the account of them fills 22 pages of letter-press of the same size. The result of this thorough treatment is to confirm the view taken by Milde; but it is left doubtful which of the two Professor Hitchcock had, if indeed he did not have both. As the title-page gives neither name of publisher nor place of publication, we conclude that this elaborate paper is privately printed. The page of preface shows that this was done at Salem, and that the figures are heliotype reproductions of Mr. Emerton's outline drawings. It is stated that "if the publication of these notes shall

ove to be of any service to fern-students [which it surely will], ey will owe it entirely to the generosity of Mr. Robinson."

A. G.

4. *Researches in regard to the influence of light and radiant at upon transpiration in plants*, by J. WIESNER (translated in nn. d. Sc. Nat., Sept., 1877, from Sitzungsb. der k. Akad. d. issensch., 1876, t. 74). Wiesner prefaces his memoir by a short storical account, of which we here give an abstract. Near the iddle of the last century, Guettard demonstrated by rude exriments that light favors transpiration. Unger, and, later, chs have supposed that the movements of stomata under the fluence of light are the cause of its action upon transpiration. rthélemy has, however, shown that the opening of stomata pends largely upon the pressure of gas in the interior of plants; en this pressure is slight, stomata can remain closed even in e light. The opening of stomata is according to him to be ked upon as an effect of the increased transpiration and not cause. Dehérain and Risler have stated that those rays of ht which are most efficient in decomposing carbonic acid, are e most active in transpiration. Lastly, Baranetzky has shown at transpiration is not always proportional to the intensity of ht. To explain this he assumes that there is, in the plant, a sitiveness to the action of light, which may be exhausted by frequent excitation.

Wiesner, in the memoir now noticed, gives a detailed account his own experiments, and presents the following conclusions. part of the light, which has traversed chlorophyll is transmed into heat; there results an elevation of temperature thin the tissues, and a consequent increase of tension of aques vapor in the intercellular spaces. The excess of vapor capes through the stomata. The rays which correspond to e absorption bands of the chlorophyll spectrum, and not the ys which are most luminous, are those which are efficient in nspiration; rays which have passed through a solution of chlohyll exert only a feeble influence upon transpiration. Other loring matters, xanthophyll, for instance, acts like chloroyll, but less powerfully. Wiesner has shown that active transation in young leaves of maize can take place when the stomata e closed; on the other hand, the transpiration of *Hartwegia nosa* was very feeble in the dark, although the stomata were dely open. Also that the dark heat-rays are less active in nspiration than the luminous rays, and that the ultra-violet ys have no influence at all; that, whatever may be the nature the rays, they act solely by elevating the temperature of the sues.

G. L. G.

5. *Ueber Botrydium granulatum*; by J. ROSTAFINSKI and M. ORONIN.—This is an interesting paper which appeared in the tanische Zeitung of Oct. 12. The investigations were carried simultaneously by Rostafinski in Strassburg and Woronin in Petersburg, the plates, which are beautifully done, being drawn Woronin. As is well known *Botrydium granulatum* is a unicel-

lular Alga of a more or less pyriform shape, from whose smaller end grows a branching root-like process by which the plant is fastened in the moist ground. Small as it is, it is amply provided with reproductive bodies as will be seen by the following: 1st. The contents of the pyriform portion may change into a large number of zoöspores each provided with a cilium. 2d. If the plant becomes somewhat dry the pyriform portion shrivels, and the root fibres divide up into a number of cells which may be transformed in either of two ways; if placed in a moist position their contents are transformed into zoöspores like those already described; or else each cell pushes out a hyaline root-like process which then grows so as to push the rest of the cell above ground forming what is known as the hypnospore. In the hypnospore, zoöpores are produced which have one cilium. 3d. By the germination of the uniciliate zoöspores individuals are produced which bear the spores proper. The latter may be either green or red, and in them are formed zoöspores which are furnished with two cilia and which, after escaping from the spore, unite in twos or some larger number so as to form what Rostafinski would call an isospore or, as is more generally expressed, a zygospore.

The cells of *Botrydium* sometimes bud out at the sides and the budding processes, after a time, send out hyaline roots and finally separate from the mother cell, forming a new individual. In this connection, we would refer to a plant which we found during the past summer at Eastport, Maine, and Gloucester, Mass., where it was not rare on rocks and wharfs at half-tide. The species seems to be identical with *Cœliolum gregarium* A. Br., found by Brown and afterwards by Pringsheim at Heligoland, in company with *Calothrix scopellorum* Ag. with which it is also associated on our own coast. The mode of growth, by means of buds, which afterwards send out a hyaline root, and then separate from the mother cell, is the same as that described in *Botrydium*. The sporiferous plant also bears a strong resemblance to *Botrydium granulatum*, and it seems that the somewhat anomalous genus *Cœliolum*, at least *C. gregarium*, should be included in *Botrydium*. W. G. F.

6. *Om Spetsbergens marina Klorofyll förande Thallophyter.* By Dr. F. R. Kjellman.

Ueber die Algen Vegetation des Murmanschen Meeres. By Dr. F. R. Kjellman.

Bidrag till kännedomen af Kariska hafvets Algvegetation. By Dr. F. R. Kjellman.

The above named articles, which are extracted from the proceedings of the Swedish Royal Academy are important contributions to our knowledge of arctic Algæ. Dr. Kjellman as botanist of the expedition to Nova Zembla under the command of Nordenskiöld has had exceptional advantages for the study of the vegetation of a region which is rarely visited. Although, of course, the number of species is small, a number of interesting *Phæosporæ* were found. Dr. Kjellman regards as a characteristic of the Nova Zembla coast, in striking contrast to that of Norway, that there is an almost entire absence of littoral *Fuci* and indeed of all littoral

cies whatever. The most prolific region was in water about
fathoms deep. W. G. F.

Felci e Specie nei Gruppi affini raccolte a Borneo. By Vin-
izo Cesati. *Prospetto delle Felci raccolte dal Signor O, Beccari*
la Polinesia. By Vincenzo Cesati, Napoli.—The former article
ms a pamphlet of forty-one pages, with four plates, in which the
iter enumerates the higher cryptogams of Borneo and describes
umber of new species. The latter article is much shorter and
itains descriptions of about thirty new species and varieties.
W. G. F.

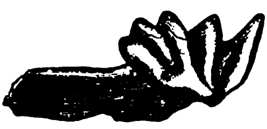
Notes on Botrychium simplex Hitch.; by GEORGE E. DAV-
PORT, 1877. Salem, Mass. 4to, pp. 22, tab. 2.—*Botrychium*
plex is a little Fern which was originally described and figured
this Journal in 1823 (vol. vi, p. 103), by President Hitchcock.
r many years it was very little known, and was confused with
eral other species of the same genus. Dr. Milde, in vol. xxvi,
the Nova Acta Acad. Nat. Curiosorum, was the first to clearly
ine it, to associate with it forms more highly developed than
specimens known to President Hitchcock, and to illustrate the
cies with figures of its several forms and variations. In the
sent paper Mr. Davenport has thoroughly discussed the plant
all its forms, and has carefully pointed out its real differences
m its nearest ally. He gives a great number of stations where
ias been discovered, and in the two plates he has had figured
ny specimens of this species and of other species which have
n mistaken for it. The whole forms an interesting and valua-
contribution to the history of North American Ferns; and it
o be hoped that the author will be encouraged to publish the
ults of his studies upon other species. D. C. EATON.

Note on the Habits of young Limulus; by ALEXANDER
ASSIZ.—Mr. C. D. Walcott has called attention to the fact
it when collecting fossils he finds large numbers of Trilobites
their back;* from this he argues that they died in their nat-
l position, and that when living they probably swam on their
ks. He mentions, in support of his view, the well known fact
it very young Limulus and other Crustacea frequently swim in
it position. I have for several summers kept young horse-shoe
bs in my jars, and have noticed that besides thus often swim-
ng on their backs, they will remain in a similar position for
urs, perfectly quiet, on the bottom of the jars where they are
ot. When they cast their skin it invariably keeps the same
itude on the bottom of the jar. It is not an uncommon thing
find on beaches, where Limulus is common, hundreds of skins
own up and left dry by the tide, the greater part of which are
ned on their backs. An additional point to be brought for-
rd to show that the Trilobites probably pass the greater part
their life on their back; and die in that attitude, is that the
ung Limulus generally feed while turned on their back; mov-
at an angle with the bottom, the hind extremity raised, they

Ann. Lyc. Nat. Hist., xi, p. 155, 1875; Twenty-eighth Report N. Y. State
seum, Dec., 1876.

throw out their feet beyond the anterior edge of the carapace, browsing, as it were, upon what they find in their road, and washing away what they do not need by means of a powerful current produced by their abdominal appendages.—*Communicated by the Author.*

10. *New Species of Ceratodus, from the Jurassic;* by O. C. Marsh.—Among the interesting vertebrate remains recently found in the Jurassic of Colorado is a tooth of a *Ceratodus*, in good preservation. The specimen is a left lower dental plate, having the inner side convex, and the outer divided into five prominent projections, which are separated by four notches. The front projection is longest, and most pointed. The plate is attached to a portion of the dentary bone, as shown in the accompanying figure. The length of this dental plate is 20 mm., and the transverse diameter 11 mm. The species is the first Mesozoic *Ceratodus* found in this country, and hence of much interest. It may be named *Ceratodus Güntheri*, in honor of Prof. A. Günther of the British Museum. The geological horizon of this species is in the Atlantosaurus beds of the upper Jurassic.—*Communicated by the Author.*



Ceratodus Güntheri.
Natural size.

IV. ASTRONOMY.

1. *The November Meteors.*—At my request, Messrs. Benjamin Vail and John P. Carr, students in the State University, kept watch last night for the November meteors. The early part of the night was too cloudy for observations, but before two o'clock this morning (the 14th) the sky had become quite clear. In one hour and fifty minutes—from 1.55 to 3.45—fifty-four meteors were counted by the two observers. This was at the rate of thirty per hour. Nearly all were Leonids—that is, the point from which they radiated was in the constellation Leo. A few of the number were as large as first magnitude stars, and left trains which continued luminous for several seconds. The appearance of so large a number ten or eleven years after the maximum displays of 1866 and 1867 is quite unexpected.

Bloomington, November 14, 1877. DANIEL KIRKWOOD.

2. *On Schmidt's Nova Cygni.*—Mr. Copeland, in a letter to the Astronomische Nachrichten, from Lord Lindsay's Observatory at Dunecht, dated September 5, says: "On September 2, 1877, I examined this star with the 15-inch refractor of this observatory. It was found to be of the 10·5 magnitude and of a decided blue tint, especially when viewed in the same field with the reddish star B.D.+42° 4184 which it precedes by about 25^s. Viewed through a low power eye-piece and a powerful direct vision prism, held between the eye and the eye-piece, the light of the star was found to be absolutely monochromatic. A single prism Browning spectroscope with a slit, but without a cylindrical lens, gave a star-like image without a trace of continuous spectrum. A few hurried measures were all that could be obtained; they indicated a wave-

length of 512 mm.; but great uncertainty attaches to this determination as a slight derangement of the spectroscope prevented the introduction of a comparison spectrum. On September 3, Lord Lindsay made thirteen measures of the wave-length, with a Grubb spectroscope giving a dispersion of 3½° from B. to Z. The very reliable result was 498·6 mm. A single measure of mine gave 496·1. It will be at once seen that the light of this remarkable star is most probably identical in wave-length with the nitrogen line 498·7, or possibly with one or both of the lines 500·4 and 495·66, (D'Arrest, Undersögelser over de nebulose Stjerner, Copenhagen, 1872) occurring in the spectra nebulæ. Lord Lindsay found 500·8 mm. and 493·5 as limiting wave-lengths between which the whole width of the line must be enclosed. Bearing in mind the history of this star from the time of its discovery by Schmidt it would seem certain that we have an instance before us in which a star has changed into a planetary nebula of small angular diameter. At least it may be safely affirmed that no astronomer discovering the object in its present state would, after viewing it through a prism, hesitate to pronounce as to its present nebulous character. Judging from the brightness of the star in the finders of 3¾ inch aperture it is probable that a refractor of 5 inches aperture would be sufficient to show the monochromatic nature of its light when viewed through a small direct-vision prism.

3. *The Report of Professor Pickering, Director of the Harvard College Observatory*, to the Visitors, Nov. 26, 1877, has been published. It has been decided to devote the large refractor to photometric work and some results of general interest have been already obtained. After describing the photometric arrangements and the details of some observations upon the satellites of Mars, the Director adds: these observations have not yet been wholly reduced, it is therefore impossible to give the final conclusion. The best idea of the light of a satellite is obtained by giving the diameter to which the primary must be reduced to render it no brighter than the satellite, or the diameter of the latter, if it reflected light in the same proportion as the planet. This is, in fact, probably the only estimate we can ever make of the true diameter of these bodies. An approximate reduction of the measurements of the outer satellite, comparing its light with that from the holes, gives its equivalent diameter at about 5·9 miles. The result of the other comparison is about 5·4 miles. This agreement is all that could be desired. The observations of the inner satellite give its diameter as 6·5 miles. The direct comparison of the two gives their relative diameters as in the ratio of 10 to 9. These figures will be somewhat altered in the final reduction. As the darker color of the outer satellite somewhat diminishes its light, it is probably safe to call it about six miles in diameter, and the inner satellite seven miles. . . . .

This photometer has since been used upon various other objects. A large number of measurements of seven of the satellites of Saturn have been obtained, including the very faint object

Hyperion, whose light is thus determined with great precision. Numerous other very faint objects have been similarly measured, especially some minute companions to bright stars. Standards will thus be established for the fainter stars, with which observers hereafter can compare other minute objects. Several asteroids have been compared in like manner, and will give some reliable data regarding the true diameters of these bodies.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Norwegian Exploring Expedition.*—The Norwegian Atlantic exploring expedition returned to Tromsöe, in Norway, by latest advices, for the purpose of refitting for the further prosecution of its work, the first period having occupied from the 13th of June until the 8th of July. It will be remembered that the object of this exploration is the discovery not merely of the more important facts in regard to the physics and natural history of the northern seas, but also of making an economical application of the same in connection with the Norwegian fisheries, which constitute so important an element in the prosperity of that nation. This is only a continuation of the enlightened measures initiated by the government of Norway many years ago, upon the results of which, indeed, we are obliged to depend for most of our information in regard to the natural history and economy of the herring, mackerel, cod and other northern fish.

An interesting point established by this first cruise of the *Vöringen*, the vessel employed, is that the minimum of temperature is not always at the bottom of the water, but often at a certain depth near the surface, its distance therefrom varying with the season or other circumstances. In the West Fiord the temperature at the surface was 45·7°; this decreased to 38·8° in sixty fathoms; and then in 140 fathoms, ten fathoms above the bottom, it rose to 41°. The explanation of this phenomenon appears to be that in winter the air is generally cooler than the sea surface, and the layers of surface water being chilled and becoming denser, sink down to a certain level, below which the temperature then usually increases with the depth. In the spring and summer the air at the surface is warmer, and the surface layer of water has no further tendency to sink. This maintains a cold stratum persistently between the surface and the bottom, which continues until by the action of storms or currents an equilibrium to a certain extent is effected.

The greatest depth found during the cruise was 1710 fathoms, with a bottom temperature of 28·4°, the lowest yet found by the expedition.—*Harper's Weekly*, *October* 27, 1877.

2. *Translation of Weisbach's Mechanics.*—Professor DuBois has translated that part of volume second, section second of Weisbach's Mechanics that treats of *Hydraulics and Hydraulic Motors.* Though complete in itself it is also intended as a continuation of Coxe's translation of volume I, and it is to be soon followed by a translation of that part of volume II that treats of *Heat, Steam and the Steam Engine.* It is hoped that the transla-

tion of volume III will soon follow. The part now issued forms a volume of lviii and 675 pages. It is published by J. Wiley & Sons.

3. *A new Treatise on Steam Engineering, Physical properties of permanent Gases, and of different kinds of Vapor;* by JOHN W. NYSTROM, C.E. 185 pp. 8vo. New York, 1876. (G. P. Putnam's Sons.)—This work contains many formulas and tables relating to combustion, steam pressure and so on, which will be useful to the practical engineer. The author makes a vigorous attack upon many of the commonly accepted terms in mechanics, as he has done in his Elements of Mechanics. A large number of new symbols and characters are introduced, the desirability of some of which may perhaps be questioned.

4. *A List of Writings relating to the Method of Least Squares*, with historical and critical notes; by MANSFIELD MERRIMAN. 82 pp. 8vo. (From the Trans. of the Conn. Acad. of Science.)—In this memoir Mr. Merriman gives over 400 titles of volumes or memoirs relating to the Method of Least Squares, beginning with Cote's rule, published in 1722, and ending with the year 1876. The work is however not a mere list of titles. He has given notes upon and abstracts of all the more important papers. About one-fifth of the titles are quoted at second hand. These are in general those of works of minor importance. The rest he has been able to examine in the libraries of Yale College. The preparation of this list with the criticisms of papers is a contribution of great value to exact science.

5. *Elements of the Method of Least Squares;* by MANSFIELD MERRIMAN. 198 pp. 8vo. London, 1877. (Macmillan & Co.)—Mr. Merriman treats the subject of Least Squares in two sections. In the first he introduces the least amount of theory possible, the object being to give and explain the practical rules. In the second he develops the theory.

6. *Royal Society.*—In their award of medals for the present year, the Council of the Royal Society have taken a wide view, for four of the five men chosen for the honor are foreigners. The Copley medal goes to Professor J. D. Dana, of New Haven, Conn., for his biological, geological, and mineralogical investigations, carried on through half a century, and for the valuable works in which his conclusions have been published. Mr. F. A. Abel, F.R.S., the newly created C.B., is to have a Royal Medal for his physico-chemical researches on gun cotton and explosive agents; and when we bear in mind the diligence and intelligence with which these researches have been carried out during many years, and the admirable papers thereupon published in the Philosophical Transactions, we may safely predict that this award will be generally approved. A Royal Medal is awarded to Professor Oswald Heer, of Zurich, for his numerous researches and writings on the Tertiary plants of Europe, of the North Atlantic, North Asia, and North America, and for his able generalizations respecting their affinities and their geological and climatic relations. For the first award of the Davy Medal, Robert Wilhelm Bunsen, of Heidelberg, and Gustav Robert Kirchoff, of Berlin, are selected,

in recognition of their researches and discoveries in spectrum analysis. This is a good beginning with a medal which is, perhaps, destined to become famous in the history of science. There will be no question as regards the custody of the golden prize, for each of the two learned professors will have a medal.—*Athenæum.*

The Silver Country or the Great Southwest. A review of the Mineral and other wealth, the attractions and material development of the former kingdom of New Spain, comprising Mexico and the Mexican Cessions to the United States in 1848, and 1853, by A. D. Anderson. 221 pp. 8vo. New York, 1877. (G. P. Putnam's Sons).

Journal of the American Electrical Society; including original and selected papers on Telegraphy and Electrical Science. Vol. i, No. 2, Chicago, 1877.

OBITUARY.

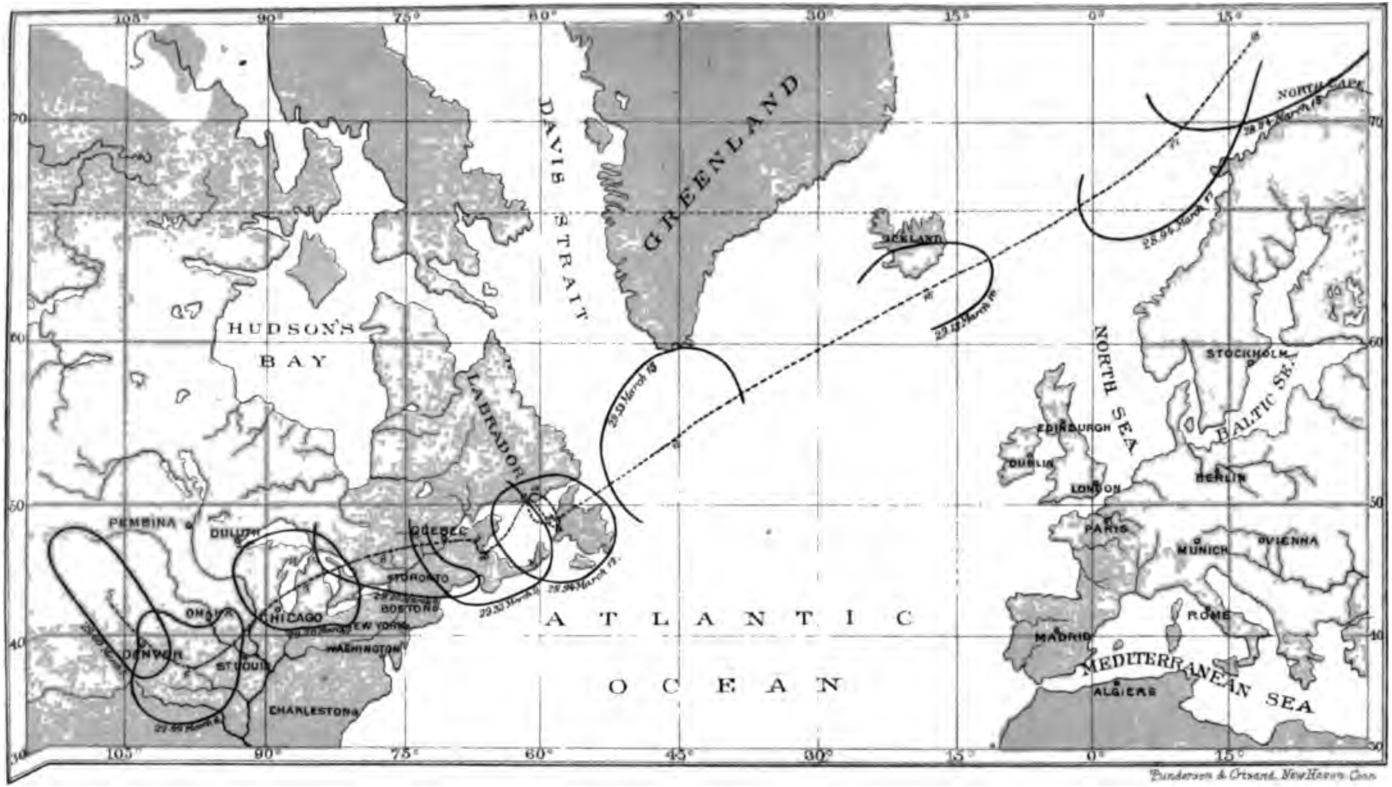
JARED POTTER KIRTLAND, M.D., LL.D.—Dr. Kirtland died at his residence in East Rockfort (near Cleveland), Ohio, December 10, 1877, at the advanced age of eighty-four years, having been born in Wallingford, Connecticut, November 10th, 1793. In scientific research and study Dr. Kirtland devoted himself especially to general natural history and zoology. His discovery of the existence of sex among the Naiades of North America was announced by his paper in volume xxxvi (1834), of this Journal. This important observation has since been fully confirmed by others, although for a time sharply contested. He also first noticed that the young of the Naiades soon after escaping from the mother are attached to some fixed object by a byssus and for the first year of their lives become selsite, (this Journal, (I) xxxix, 164) Dr. Kirtland removed to Ohio in 1823, having graduated in medicine at Yale College in 1815. He conducted the survey of the Natural History of Ohio under the first Geological survey of that State in 1848. The large collections then made at his own cost he subsequently devoted to founding the Cleveland Academy of Natural History, the State of Ohio refusing to reimburse him the expenses of the collection. His catalogues of the fishes, reptiles, mollusks and birds of Ohio form part of the published results of that survey, and subsequently his descriptions of the fishes with figures drawn by his own hand were published in the Journal of the Boston Society of Natural History.

Dr. Kirtland was a man of untiring industry, devoted to the duties of an arduous practice in medicine, and those of a professor of the theory and practice of his art in more than one Medical College. He was a zealous cultivator of plants and was successful in producing many new varieties of fruits. His large and generous nature made him a philanthropist and patriot and endeared him to a large circle of admiring friends, to whom his stores of exact and varied knowledge afforded an unfailing source of enjoyment. He retained his intellectual powers undimmed to the close of his long life.

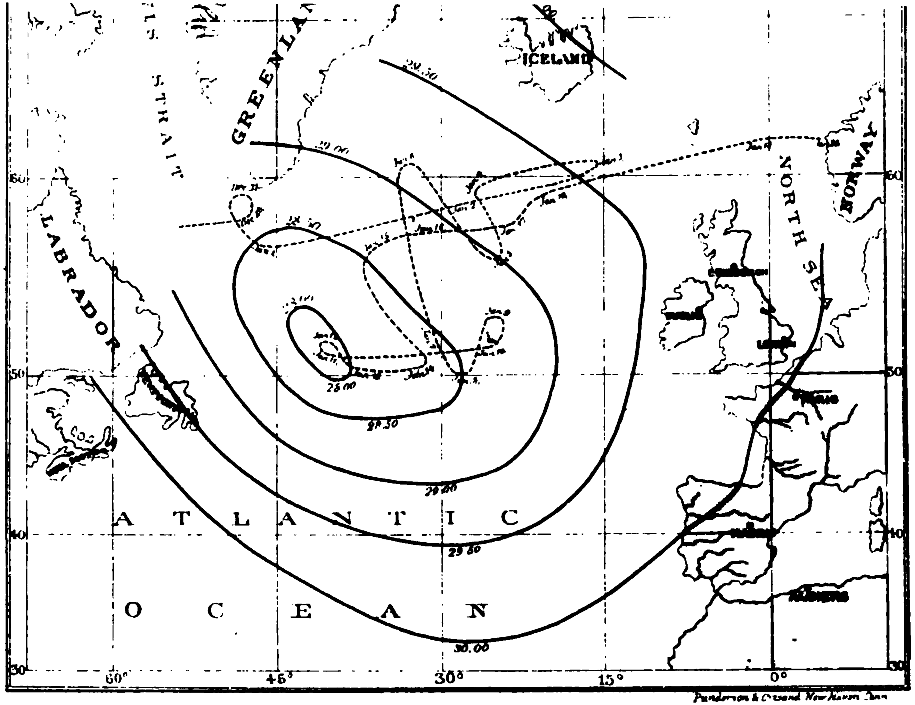
Dr. Kirtland was one of the subscribers to the first issue of this Journal in 1818, and his name has stood on its books to the close of the last year, being, so far as appears, the last member of that original number. B. S.

PLATE I.

STORM OF MARCH 5-18, 1874.



Punderson & Crisand, New Haven Conn.



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AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. VIII.—*On the Photometric comparison of Light of different colors;* by Professor O. N. ROOD, of Columbia College.

[Read before the National Academy of Sciences, Oct. 24th, 1877.]

THE comparison of the intensities of light of different colors has long been considered one of the most difficult of photometric problems, but by the use of very simple means, I have recently made a series of measurements of this character, which may not be without interest to those whose studies lie in this direction. The luminosity of card-board painted with vermilion, was, for example, measured as follows: a circular disc of the vermilion card-board was attached to the axis of a rotation-apparatus, smaller circular discs of black and white card-board being simultaneously fastened on the same axis, so that by varying the relative proportions of the latter, a series of grays could be produced at will.

The compound black and white disc was now arranged so as to furnish by rotation a gray which was *decidedly darker* than the vermilion; this gray tint was then gradually lightened, till the observer became doubtful as to the relative luminosities of the red and gray discs; the angle occupied by the white sector was then measured. Next, a gray decidedly *more* luminous than the vermilion was compared with it, and diminished in brightness till the observer again became doubtful, when a second measurement was taken. All this time the manipulation was performed by an assistant, the experimenter giving directions, but remaining in ignorance of the results to the end. The mean of ten such experiments assigned to the vermilion disc a luminosity of 23·8, that of white card-board being taken as 100. In this experiment and in all those that follow, proper corrections were made for the amount of white light reflected by the black disc, this having been previously ascertained, in a manner which will be described in a future communication.

In order to test the correctness of the final result, the luminosity of a blue-green disc, correctly complementary in color to the vermilion, was next measured in the same way; it proved to be 26·56. The vermilion and blue-green discs were then combined, according to Maxwell's method, so as to obtain a pure gray by rotation, and the angular proportions of these colored surfaces, and the value of the gray in terms of white and black card-board, measured. The gray thus obtained had a luminosity of 24·54, that of white card-board being 100. Next, the value of this same gray was *calculated* from the measured luminosities of the two colored discs, and the proportions of these colors required to produce a pure gray by mixture on the rotation apparatus; the calculated value was 25·47.

This agreement proves the correctness of the photometric comparison, and also of Grassman's assumption, that the total intensity of the mixture of masses of differently colored light is equal to the sum of the intensities of the separate components, which, so far as I know, has not before received an experimental confirmation.

Corresponding measurements were made with a green, and its complementary purple disc; also with a blue, and its complementary yellow disc; the results are given below.

	Luminosity.	Gray (observed.)	Gray (calculated.)
Vermilion,	23·8	24·54	25·47
Blue-green,	26·56		
Chrome yellow,	80·3	54·51	53·92
Cobalt blue,	35·38		
Green,	41·19	24·94	26·56
Purple,	14·83		

ART. IX.—*The Geological Commission of Brazil. Additions to the Echinoid fauna of Brazil;* by RICHARD RATHBUN.

THOUGH the Geological Commission has busied itself in investigating the marine fauna at almost every point on the coast of Brazil where its labors have extended, yet in a group like that of the *Echini*, the different shore species of which are so readily found and collected by all travelers, it has been able to add very little that is new. In its collections, however, are three species which do not seem to have been recorded from Brazil before. They are: *Diadema setosum* Gray, *Hipponoë* *esculenta* A. Agassiz, and *Mellita sexforis* A. Agassiz. According to Mr. Alex. Agassiz in his "Revision of the Echini," the first mentioned species is almost world-wide in its distribution, occurring in the seas between North and South America and Mexico, among the West Indies, the islands off the northwest of Africa, and also in many places in the Pacific and Indian Oceans. We cannot yet record it from the mainland of Brazil, but it was found in great abundance by Mr. J. C. Branner, on the shores of the island of Fernando de Noronha, situated nearly 200 miles to the northeast of Cape St. Roque.

The second species, *Hipponoë esculenta*, was obtained by Mr. Branner from the same locality and also from the mouth of Rio Formoso, about twenty miles south of Cape St. Agostinho, in the province of Pernambuco. In its previously known range, as recorded by Mr. Agassiz, it was limited to the main shores and islands of the seas between North and South America, and to the Bermudas.

The third and last species mentioned, *Mellita sexforis*, was dredged from the shallow water inside of Fort Villegagnon, in the harbor of Rio de Janeiro, by Professor E. Selenka, who has kindly presented the Commission with several specimens. They are very numerous in this one locality, but, so far as we know, have not been obtained from any other part of Brazil. Of this species, Mr. Alex. Agassiz says that it has a great range in depth, from shallow water to 270 fathoms, but has not been found over an extended geographical area, as it has been recorded from the southern United States, the West Indies and Bermudas, only.

In addition to the two species above mentioned from Fernando de Noronha, there were obtained from the same locality three other species already known from the coast of Brazil: *Cidaris tribuloides* Bl., *Echinometra subangularis* Desml., and *Toxopneustes variegatus* A. Agassiz. *Cidaris tribuloides*, though very rare indeed along the coast of Brazil and represented in the collection of the Commission from there by only a few fragments, was exceedingly abundant at Fernando de Noronha, occurring there in thousands. It never seems to attain the size of the specimen from Cape Verde Isles, figured by Agassiz (op. cit., pl. 1*d*), and varies much in the length and strength of the spines, which are usually more or less overgrown with calcareous sea weeds. Though Fernando de Noronha is not 200 miles distant from the main coast, yet there intervenes a great depth of water, as ascertained by recent exploration; the marine fauna of the island, however, seems to be very similar to that of the coast.

The following list probably contains all the littoral species of *Echini* at present known from Brazil, all of which, with the exception of *Strongylocentrotus Gaimardi* A. Agassiz, have been found by the Geological Commission. The localities at which they were obtained are so nearly those recorded by Mr. Alex. Agassiz in his Revision of this group, that it would be of no value to enumerate them again here. The species are as follows:

Cidaris tribuloides Bl.,
Arbacia pustulosa Gray,
Diadema setosum Gray,
Echinometra subangularis Desml.,
Strongylocentrotus Gaimardi A. Agassiz,
Toxopneustes variegatus A. Agassiz,
Hipponoë esculenta A. Agassiz,
Clypeaster subdepressus L. Agassiz,
Mellita testudinata Klein,
Mellita sexforis A. Agassiz,
Encope emarginata L. Agassiz.

It has been noticed that *Mellita testudinata* is brilliantly phosphorescent at times, when it has been removed from the water for a few hours, though it be still wet and apparently alive. The phosphorence is very apparent over the entire body. I am indebted to Mr. Alex. Agassiz for kindly examining and verifying the determinations of a collection of the above *Echini* sent to the United States, excepting *Mellita sexforis*, which has been found only very recently.

ART. X.—*Undulations observed in the tail of Coggia's Comet,* 1874; by L. TROUVELOT.

ON the evening of July 21, 1874, at 9h 0m, the moon being in her first quarter, and the sky remarkably clear even close to the horizon, my attention was attracted by a bright ray of light darting from the northwestern horizon, way up in the constellations. Taking it for an auroral phenomenon, I went in for the spectroscope; but on my return, after a few seconds, to my disappointment I found no more trace of it. Soon, however, it reappeared, and darted up in an instant after the manner of certain auroral rays, and vanished again after ten or fifteen seconds. I then became aware of my error, and found with surprise that the phenomenon was taking place in the tail of Coggia's Comet, the head of which was then plunged under the horizon.

During the whole time that I observed this interesting phenomenon, I saw the comet's tail shortening and extending, lightning up and extinguishing like the rays of certain auroras. Extended undulations, rapid vibrations, ran along it in succession from the horizon to its extremity, giving it the appearance of a fine gauze wavering in a strong breeze. The pulsations and the waves of light were of unequal duration; some being rapid, while others lasted a longer time. For over one hour the comet's tail kindled and extinguished more than one hundred times; the extinction being sometimes so complete that it was impossible to see any trace of the comet; while sometimes it became so bright, that in spite of the light of the moon, it could be distinguished easily in all its contours, even to its very extremity, which was then a little to the south of γ Ursæ Majoris.

Be it coincidence or not, at the moment that this phenomenon was occurring, a strong magnetic perturbation was going on in Cambridge, where the declination needle oscillated through an angle of 1° 27′, although no auroral light was seen; and by the kindness of Mr. Cleveland Abbe, of the Signal Corps, I learn that no aurora was reported for that night.

It is not a new thing to see vibrations and pulsations running along the tail of comets. Many observers have seen this phenomenon; among others Longomontanus, Vandelin, Snellius and Father Cysat, who are reported to have seen undulations taking place on the border of the Comet of 1618, as if it was agitated by the wind. Hevelius observed analogous motions in the Comets of 1652 and 1661. Pingré asserted that he distinctly saw in the long tail of the Comet of 1769, "des ondulations semblables à celles que les aurores boréales présentent."* According to Winnecke, from the 5th to the 12th of October, 1858, the rays forming the superior part of Donati's comet spread and contracted suddenly like the rays of the aurora.

Cambridge, Jan. 5, 1877.

ART. XI.—*Sudden extinction of the light of a Solar Protuberance*; by L. TROUVELOT.

ON the 26th of June, 1874, while making my daily observations of the sun with the spectroscope at the Harvard College Observatory, I saw an unusual phenomenon, which may be worth recording. The narrow slit of the instrument was directed on the preceding side, about 270°, just above a group of spots which was then very near the limb, when I saw a brilliant protuberance partly projected on the spectrum, on the side of the rays of less refrangibility. In shape, this hydrogen flame resembled an elongated comma, having its acute extremity directed toward the sun, where it terminated just a little above the chromosphere. The chromosphere under this protuberance formed several slender and acute aigrette-shaped flames, none of which, however, reached it. The large prominence, which was slightly inclined to the limb, had a height of 3′ 37″, and about 8″ in its greatest width.

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* Arago, Astro. pop., vol. ii, p. 439, Paris, 1855.

When the slit was set wide open, so as to allow the whole protuberance to be seen between its jaws, the comma-shaped flame appeared perfect, and showed plainly its texture. But when it was observed with a narrower slit it became partly invisible on the C line; only a short and jagged portion being seen in it on the red side. When the slit was carried along the protuberance by means of its screw, the portion visible on the C line did not remain constant, but either extended or contracted of a small quantity; the maximum portion visible on the C line never being more than one-fourth the width of the slit, while sometimes it was not seen at all on this line.

The portion of the protuberance projected on the spectrum was considerably more brilliant than the spectrum itself, and about one-third only of its whole length was visible. As the slit was carried along it, the visible parts became invisible near the C line, and invisible parts appeared on the spectrum; and the area of the visible portion either contracted or extended, when seen in different parts.

I had been observing this phenomenon for eight or ten minutes, when, while looking at it with the slit wide open, the flame suddenly vanished, at $10^h\ 30^m$, no traces of it remaining. As no motion of any kind, no extension, no contraction, could be perceived before or at the moment this phenomenon took place, and as the light did not go out of it gradually, but as suddenly as a flash of lightning, it does not seem that a change of position was the cause of its disappearance, but rather because the light which rendered it visible abandoned it in an instant.

According to theory, this protuberance was moving rapidly away from the earth at the moment of the observation, as it was projected upon the less refrangible side of the spectrum; yet this would fail to explain its sudden disappearance, since for this it should have moved out of sight with an unconceivable velocity.

For over half an hour I watched attentively the same spot in expectation of seeing the flame reappear; employing for this a narrow and a wide slit in succession, but with entirely negative results. I saw no more traces of it, although the small aigrette-shaped flames of the chromosphere, which were still visible, indicated the exact place where it had vanished, and where very probably it still existed, but now as a dark protuberance.

On several occasions I have seen the light abandon a protuberance gradually, but never so suddenly and on such a grand scale; and sometimes I have seen also the light gradually illuminating protuberances which were invisible before, something after the manner of clouds in our atmosphere, lighting up and fading into darkness by the appearance or disappearance of the sun. Of course, the illumination of dark solar protuberances cannot be conceived as being due to the reflection of light, as in the case of the clouds in our atmosphere: it is the protuberance itself which is rendered luminous by some change taking place in it. These observations would seem to indicate that on the sun there are sometimes dark and non-luminous protuberances, which may cause the spots of absorption often observed in the vicinity of spots.

The phenomenon of the gradual illumination of a protuberance was observed in 1869, at Des Moines, Iowa, during the total eclipse of the sun, by Professor William A. Rogers, who accompanied Dr. C. H. F. Peters, on the Litchfield Eclipse Expedition. Professor Rogers was observing a. large protuberance on the sun with a 9-inch-aperture refractor, when he saw several protuberances form gradually in the vicinity of the large flame, and at a considerable height above the chromosphere.

The projection of the hydrogen flames on the spectrum is not a very rare phenomenon during the period of maximum of sun-spots, and it has been observed several times. However, it may be worth while to record a characteristic case of projection, accompanied with remarkable changes of form, and a visible motion of the protuberance.

On Sept. 10th, 1872, at $12^h\ 33^m$, I was observing a small narrow flame forming an arch on the chromosphere, the height of which was equal to 36″. Nothing remarkable was to be seen in this protuberance, although it was in the vicinity of a group of spots which was then very near the eastern limb of the sun; but, two minutes later, one of the extremities of the arch reposing on the chromosphere was suddenly detached from the limb, springing up like a distended bow, ascending in an instant to a height of 70″, then appearing straight and rigid, but twisted like a rope. For a few seconds, it continued to ascend, at the same time growing wider; and at $12^h\ 37^m$, it had attained its maximum altitude of 118″. It was then slightly curved. At $12^h\ 43^m$, the force which had carried it up began to give way, and it then descended rapidly toward the sun, folding upon itself in two places, while at the same time it became narrower. At $12^h\ 45^m$, it had reached its former height; and soon after it sunk to a level with the chromosphere and was lost in it.

At the same instant that the arc of hydrogen was distended, it was seen projected on the spectrum opposite the sun, towards the violet. The figure of this protuberance appeared exactly the same, whether it was projected on the spectrum or seen between the wide-open jaws of the slit. However, when the slit was narrow, the flame became invisible on the C line, although it remained projected on the spectrum. When the protuberance, after having reached its greatest altitude, de-ascended rapidly toward the sun, it remained projected on the spectrum just as before, although the descending motion was apparently in a contrary direction to the ascending one; but this did not seem to affect the position of the flame on the spectrum.

Cambridge, January 12, 1877.

ART. XII.—*The Moon's Zodiacal Light;* by L. TROUVELOT.

DURING the evening of April 3, 1874, the "Zodiacal Light" was particularly brilliant; especially close to the horizon, where it appeared as a segment of a circle, having an irregular wavy outline, giving it a vague resemblance to the beams of a faint aurora. Although the sky was clear, it was found impossible to observe with the telescope on that night, on account of the great disturbance of the atmosphere. At 9^h 45^m, the declination needle indicated a very strong magnetic perturbation in Cambridge, oscillating through an angle of 3° 22′. However, no aurora was visible at this time, although the phenomena usually attending them were manifested during the evening by the tremulous appearance of the telescopic images.

While going home, I remarked in the east a strange conical light rising obliquely from the top of the roof of a building, behind which the moon, then about 15° or 20° above the horizon, was concealed from view. By going away from the building, the conical light, which closely resembled the tail of a comet, became brighter and brighter as it approached the moon, upon the western limb of which it rested. The base was at least as wide as the diameter of the moon; but it extended beyond, on each side, by a fainter light, which gradually vanished in the sky. The extension of this luminous appendage I estimated to be equal to eight or ten times the moon's diameter. It was not readily visible when the moon was in sight, as the brilliant light of our satellite overpowered its dim brilliancy. The axis of this appendage was found to be coincident, or nearly so, with the ecliptic; and its line prolonged in the west passed a little to the north of Jupiter.

The phenomenon had been observed for about fifteen minutes. when it gradually faded away until it almost totally disappeared five minutes later, although the sky was clear. A quarter of an hour after, the sky was overcast with dense vapors, which continued for nearly an hour.

At 11^h 0^m the sky had cleared up, and the moon shone brightly. The luminous appendage was still visible, and even appeared more brilliant than before. In order to ascertain whether this appendage was visible only on one side of the moon, or if it was seen on the other side, I went under the piazza of my house, and placed myself in such a position as to have the moon concealed by its upper part, the sky below being visible. As I expected, a similar appendage was observed on the eastern side of the moon, exactly opposite the western one; the axis of both wings, passing through the moon's center, being in the plane of the ecliptic.

Although at this moment no auroral light was seen in the north, yet, up in the zenith, there were evident signs of it, as luminous vapors assembled there and rapidly dissolved, arranging themselves into bands radiating from a center after the manner of the crown of bright auroras. At 11^h 20^m, all traces of the luminous vapors in the zenith had vanished; and at the same time the appendages of the moon were almost totally invisible, although the sky remained clear.

The fact that the zodiacal light had been unusually brilliant during this evening, and that the two luminous appendages of the moon resembled it in shape and appearance, and were situated in the same plane, seems to indicate that the two phenomena are of the same order; while the magnetic perturbation and the auroral phenomena connected with the variation of brightness observed in the moon's appendages would seem to indicate some kind of connection between the zodiacal light and the aurora. The result of my observations of the zodiacal light and the aurora during the last seven years also seems to indicate some such connection, as when the zodiacal light was observed to be particularly bright, it has generally been followed by auroral phenomena. But only a long series of observations in this direction can solve the problem.

Cambridge, Nov. 2, 1877.

ART. XIII.—*Chemical Equivalents and Atomic Weights considered as bases of a system of Notation;* by C. MARIGNAC.

(**Translated from** Moniteur Scientifique of Quesneville for September, 1877, by Mr. P. Casamajor.)

THE Academy of Sciences of Paris has witnessed lately, at several of its sittings, an interesting discussion, in which several eminent chemists, among its members, have taken part.* This discussion related to two questions which have often been brought before it, and which will probably be brought before it again many times.

One subject of discussion was a principle, stated in 1811 by

* Messrs. Sainte Claire Deville, Würtz, Berthelot, Fizeau.

an Italian philosopher, Avogadro, on the equality of the molecules of all bodies in a gaseous state. This principle is often placed in opposition to the law of Gay Lussac, on the simple relations which exist between volumes of gases, capable of combining with one another, which law was established a few years before, and which, to tell the truth, is not in contradiction with the hypothesis of Avogadro. From this question arose another, on the relative merits of the chemical notations, expressed in equivalents or in atoms.

For the present, I will not discuss the first of these questions. The truth of the principle of Avogadro can only be admitted on the condition of supposing that the atoms of simple gases cannot exist in a free state, but are welded together in pairs, forming molecules occupying two volumes, like the molecules of compound bodies. Exceptions should be made for mercury and cadmium, whose molecules are formed of only one atom, and for phosphorus and arsenic, whose molecules must contain four atoms.

This hypothesis is not absurd in itself. It may account for certain chemical facts, such, for instance, as the greater energy of action that bodies possess in a nascent state, or before their atoms have combined two by two to form molecules; also for the ease with which certain reactions take place, as pointed out by M. Würtz. It also explains several physical facts, such as the equality of specific heat for the same volume of simple or compound gases, whose molecule is formed of two atoms, as carbonic oxide, hydrochloric acid. It found lately an important confirmation in the researches of Messrs. Kundt and Warburg* on the specific heat of vapor of mercury, which show that this heat agrees with the mechanical theory of heat for monatomic gases, and that this agreement does not exist for other simple gases. We must acknowledge, however, that these considerations do not constitute sufficient proofs.

On the other hand, there are some compound bodies whose vapor densities are in contradiction with the principle of Avogadro. We should be forced to admit that all these compounds suffer decomposition when they seem to be reduced to vapor, so that, instead of measuring their volume, we measure that of their elements, or of the products of their decomposition. Although this decomposition has been ascertained in some cases, it has not in all.

As may be seen, the principle of Avogadro gives rise to serious objections, and, without being convinced of its worthlessness, like my eminent friend, M. Deville, I acknowledge that it is as yet but an hypothesis, in contradiction with facts, which have not been satisfactorily explained. But, I repeat it, I have no wish to enter into this discussion at present, as it would require to be extensively developed, and it can only be definitely settled by long and difficult experiments. I have recalled this discussion because its solution must exert a certain influence on chemical notations, although the connection between the two questions is not necessarily very close.

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* Berichte der deutschen Chemischen Gesellschaft, 1875, p. 945.

As to the best system of notations, it may be necessary to explain why such a question is raised and can only be raised in France. In every other country the question has solved itself gradually, as chemists have, one after another, accepted the atomic notations, and given up the formulas by equivalents in their writings and in their teachings* In this gradual manner, in almost every country, by the successive assent of the great majority of chemists, atomic formulas have been substituted for the others without any formal struggle. In France, however, it has been very different. I am not aware whether the regulations of the University† forbid a professor from adopting the method of instruction which he thinks best, or whether teachers adopt a uniform system from the belief that otherwise their pupils would be placed in a relatively inferior position, if they did not adopt the system most in favor with examiners; at any rate, such an important change, as the introduction of a system of chemical notation, can only be generally introduced when it has been judged necessary, not only by the majority of teachers, but also by the Superior Councils, which govern the University, and, in these, chemists are not the only persons who have influence. It is easily understood that, under these conditions, the partisans of both systems wish to have them discussed in the presence of the scientific body which has the greatest authority, with the object of maintaining the established system, or of introducing the other.

To enter into this discussion, it is doubtless advisable, in the first instance, to define what is understood by these equivalents and these atomic weights, which are placed in opposition to one another.

As to equivalents, I see that M. Berthelot tells us that "their definition is a clear conception." Unfortunately he did not give this definition, and I confess that I do not know of any, at least of any that is precise and general. Doubtless when we compare, with one another, elements, such as chlorine, bromine and iodine, the definition of their relative equivalents is perfectly clear, as the term equivalent is its own definition. But when we deal with bodies which have not a similar analogy, and particularly if they do not perform the same functions, the idea of equivalence has no meaning. I defy anybody to give a general definition of equivalents which justifies the weight 14, adopted for nitrogen. In volume, it corresponds to the equivalents of hydrogen and of chlorine, but it has not the same chemical value. It has the same chemical value as the equivalents of phosphorus and arsenic, but it does not occupy the same volume. Moreover, it corresponds neither in volume nor in chemical value to the equivalents of oxygen, of sulphur and of most metals. Why then should this number exist?

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* Fresenius still considers the formulas in equivalents as the best.—*Translator.*

† The entire educational system of France is consolidated under one organization called the University, comprising faculties of letters, medicine, law and theology, *lycées* for secondary instruction and schools of primary instruction.—*Translator.*

If, instead of starting from a general definition, which does not exist, we try to find the meaning of equivalents in the methods employed in determining them, we are led to the following conclusion:

It is proved by experience that we may assign to a body, be it simple or compound, various weights, multiples of the same number, and that these weights express the proportions according to which all bodies combine with one another. We may choose one of these weights to express the equivalent of the body. All combinations may then be represented as the union of a certain number of equivalents of the elements, and, if the equivalent is represented by a symbol (in general the first letter of the name of each element), combinations may be represented by formulas which are not generally complicated. This is, after all, the only condition required of equivalents, and hence the only general definition, although not very precise, which can be given is that the equivalent represents for every element or compound body one of the weights which may combine with other equivalents. Theoretically it matters little which of the weights is chosen. Practically, however, one of the weights is preferred, taking as a guide one of the following rules, which cannot be considered as very rigid, as they do not all lead, in all cases, to the same result:

1. When bodies are analogous, and have the same chemical character, their equivalents are represented by the weights which replace each other in analogous combinations. Let us note, however, that this rule is not followed for compound bodies, such as bases and acids, whose so-called equivalents are weights which often have very different values of combination, and we are thereby led to very singular anomalies of statement, such as these: two equivalents of alumina corresponds to three equivalents of magnesia; one equivalent of phosphoric acid to three of nitric acid, &c. In reality the fundamental principle of equivalents has been entirely abandoned for compound bodies, and, in its stead, a method has been adopted, which has been borrowed from the atomic

theory, by taking for their weights the sum of the equivalents of the elements which they contain.

I believe I am not in error when I affirm this, as M. Berthelot* says: "One equivalent of phosphoric acid corresponds to three equivalents of nitric acid, when it forms a tribasic phosphate.

2. Equivalents are chosen in such a way that compounds, which offer the greatest analogies, are represented by similar formulas. This principle served as a guide in determining the equivalents of aluminum and of copper. It is often in contradiction with the preceding. For instance, aluminum and magnesium, which are both powerful deoxidizing agents, do not replace each other in the proportions indicated by the equivalents adopted for these two metals.

3d. When neither of these rules is applicable, or when they lead to complicated formulas, the equivalent of a body is chosen in such a way as to give the simplest possible formulas for its most important combinations. This rule justifies the adoption of the equivalents of nitrogen, phosphorous, arsenic and of some other elements.

We may see by the above that the equivalents constitute a purely conventional and arbitrary system, without any scientific value.

The explanation I have given of equivalents is somewhat different from that which my illustrious teacher, M. Dumas, gave in his lessons of chemical philosophy. This eminent chemist took as his starting point the equivalents of bases, as determined by their true chemical equivalence, founded on the same quantity of oxygen contained in the base. The equivalents of acids are rigorously deducted from the weights necessary to neutralize an equivalent of base. Afterwards, he seeks to establish the equivalents of the elements by considerations which, he acknowledges, are often arbitrary. This method of determining equivalents however, has been either never adopted, or entirely abandoned, doubtless because it led to formulas which are inadmissible. I have given the meaning of equivalents, such as they have been adopted, and not such as they might have been.

We may now consider the definition of atomic weights. If the precise definition of equivalents is impossible, while their determination is comparatively easy, for they are adopted by arbitrary rules, it is the reverse with atomic weights.

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* Meeting of the Académie des Sciences of June 4th, 1877. I cannot in any manner accept what he says, in the same place, that this equivalent of phosphoric acid corresponds to one equivalent of nitric acid in monobasic phosphates, or two equivalents in bibasic phosphates. To admit such expressions, we must deny to water the part that all chemists attribute to it in these salts, since the publication of Graham's researches.

If we refer to the fundamental hypothesis of the atomic theory, which supposes that the divisibility of bodies is not indefinite, but that they are formed by the agglomeration of excessively small but indivisible particles, or atoms, the theoretical definition of atomic weights is of the simplest, as they are the relative weights of these ultimate particles. But, however simple the definition may be, the determination of the weights is surrounded with great difficulties.

The hypothesis of the existence of atoms accounts in such a simple manner for that of chemically equivalent proportions for elements which play the same part, that we are naturally led, at first sight, to consider these proportions as representing their relative atomic weights, although this consequence is not rigorously necessary. It is evident, however, that as neither this consideration of chemical equivalence, nor any other consideration drawn from chemistry alone, has led to a complete and logical system of chemical equivalents, we cannot by such considerations be guided in the choice of all the atomic weights, and as these, on account of the hypothesis that is made on their nature, cannot be arbitrary, like equivalents, it has become necessary to study the physical properties of the elements and of compound bodies to find motives for this determination of the atomic weights. Among the properties which can be appealed to, the most important are the densities of gases and vapors, the specific heats and isomorphism.

I acknowledge that in some very rare cases these three orders of physical properties do not lead to the same result, and I agree with Mr. Berthelot that between these three data we must make a choice. I am, however, in complete disagreement from him in the conclusion that I draw from this. If he does not say so expressly, his whole argument proves that, in his opinion, no account is to be taken of these physical properties, when they disturb the usage established for weights that have been adopted for a long time in chemical notations. On the contrary, I think that great account should be taken of these physical properties, and that when they all agree we must have no fear of modifying a few formulas which have only long usage in their favor, particularly if the necessary modification is unimportant. If, moreover, the physical properties do not agree, it is necessary to study the facts with the greatest care, and see if, in some cases, a disagreement can be explained and then choose the weight which agrees the best with the general properties of the elements and its combinations.

Is it impossible to do this? The best proof that it is not, and that there is even no serious difficulty in determining the atomic weight which agrees the best with the physical properties, is to be found in this circumstance that there is no disagreement among chemists, who accept this system of notation, as to the atomic weights, except for a few bodies that are not, as yet, sufficiently known; whose physical properties have not been sufficiently studied, and for which, besides, the idea of equivalents is quite as uncertain as that of atomic weights.

I am perfectly aware that the majority of chemists, who have adopted atomic formulas, believe that they are now able to give a rigorous definition of atomic weights. Starting from molecules, which they define as the smallest quantity of a body, simple or compound, which can exist in the state of liberty; admitting as an axiom the principle of Avogadro, which states the equality of volume of all molecules in a gaseous state, from which may be deducted their relative weights, they define the atom as the smallest quantity of a body which may enter into the composition of a molecule. This definition allows them to determine the atomic weights with certainty, at least for those bodies that enter into volatile combinations. I have not given great weight to this consideration because, not more than Messrs. Deville and Berthelot, do I regard the principle of Avogadro as absolutely demonstrated. But I wish it to be specially noticed that there is not, so far as I know, a single case in which the application of the above definition of atomic weight has been used to change an atomic weight previously obtained by considerations based on the physical properties. Perhaps I should except boron and silicon; but the atomic weights of these elements had never been considered as firmly established, nor indeed had their equivalents. This observation might be appealed to as the strongest proof of the accuracy of the definition of atomic weights, but I have no wish to admit it, as constituting a sufficiently sure base for the determination of atomic weights.

I have here to answer an objection, which I acknowledge to be serious, and which I believe is at the bottom of the opposition of M. Berthelot. The atomic weights rest on an hypothesis which has never been, and, in fact, can never be demonstrated, which many scientific men do not consider as verisimilar, that of the existence of atoms.

I am nearly ready to agree with M. Berthelot in his opposition, and I have certainly no idea of defending the atomic theory, but merely the chemical notations founded on the atomic weights. My answer to the objection stated above is that the existence of atoms is only useful in justifying the name of *atomic weights*, which, for my part, I would very willingly have replaced by any other. I know of no case in which an atomic weight has been determined by a method founded on the indivisibility of atoms; consequently we may consider atomic weights as entirely independent of this indivisibility.

In reality, I consider atomic weights, and I believe that many chemists agree in this, as being only equivalents, in the determination of which arbitrary conventions have been replaced by scientific considerations, based on the study of physical properties.

Let us now sum up the advantages that atomic notations present from this point of view.

For the elements, in the first place, the atomic weights represent equal volumes of all simple gases, so that their ratios of combination in volumes are directly expressed by atomic formulas, while the formulas in equivalents do not offer this advantage. This law presents some exceptions for vapors, in the cases of phosphorus, arsenic, mercury and cadmium, but the same divergence exists for equivalents.

Atomic weights are exactly proportional to the specific heats of simple gases, that are not liquifiable, which agreement does not exist for equivalents. According to the law of Dulong and Petit, the specific heats of the atoms of all simple bodies, either solid or liquid, are nearly the same, except for three bodies, carbon, boron and silicon, whose physical properties offer numerous irregularities, and in which the specific heat varies with the temperature in a manner unknown in other bodies. Equivalents do not offer this concordance. I will not insist on the objection raised by M. Berthelot, and founded on this, that the equality of specific heats of atoms is far from being absolute, as he was sufficiently answered by MM. Würtz and Fizeau. I will merely add that if we only admitted physical laws that are absolute, we should have to reject them all. Even the law of volumes of Gay Lussac would have to be dropped, as it has been ascertained that all gases have not the same coëfficient of expansion, so that the existence of simple ratios in combinations by volume are not strictly accurate.

As to compound bodies, the molecular formulas, based on the use of atomic weights, present the same advantages, perhaps to a higher degree, when we compare them to the formulas in equivalents.

The use of atomic weights allows us to simplify the formulas of a great number of compounds by dividing them by two. Particularly is this the case with organic compounds. Not only does the formula become simpler, but there is an important advantage gained, that the formulas of almost all compounds correspond to the same volume, which is double the volume of the simple atom. The only exceptions are for a very limited number of bodies, generally belonging to types of complex composition, such as salts of ammonia and of bases derived from ammonia; even for these it has not been proved that they are not regulated by any law, even if M. Deville is right in thinking that the irregularities they present are not due to the decomposition of their vapors. On the other hand, the formulas by equivalents teach us nothing on the vapor densities of compound bodies, as their equivalents may correspond to two, four or eight volumes of vapor, perhaps even of six, if the old equivalent of silicon is kept, as is done by many of those who prefer the notations by equivalents. Molecular formulas also agree with the specific heats of compound bodies in the solid state. According to the law of Woestyn, molecular heats are proportional to the number of atoms contained in the molecule, which law has the same degree of approximation as that of Dulong and Petit. Formulas by equivalents do not show these properties.

Finally, the system of notation, based on atomic weights, gives the explanation of several cases of isomorphism which are incomprehensible with the notation based on equivalents. For instance, in the case of perchlorates and permanganates, and in the case of chloride and sulphide of silver when compared to protochloride and protosulphide of copper. I may also recall that it was by considerations of the same kind that I was led to discover oxygen in fluorine compounds of niobium, where its presence had not been suspected, and that the formulas of these compounds, expressed in equivalents, would never have suggested this idea.

In presence of these advantages, we may ask: what are those that are offered by the system of equivalents and its resulting notation? I believe I can indicate two.

In the first place, as the system is conventional, it does not of itself contain any necessary reason for changes, and it may remain invariable. As there was no serious motive for choosing the number fourteen as the equivalent of nitrogen, rather than seven, which would have given it the same volume as oxygen, or $\frac{14}{3}$ which would have accounted for its value of combination toward hydrogen and the metals, we may readily believe that there will never be a sufficient motive to replace it by one of these numbers. The determination of equivalents not being governed by any fixed rule, they will not be necessarily modified when we come to have a more accurate knowledge of the properties of bodies.

In the second place, as, in their determination, no account is taken of the physical properties of bodies, greater attention can be given to their chemical equivalence, when it exists. This presents some advantages in practical chemistry.

These considerations are doubtless of some value; but if we examine things a little closer, we may easily see that, in this respect, there is really very little difference between the two systems.

It is true that there was a time when atomic weights had to be changed, and it is doubtless, on this account, that atomic weights were dropped and equivalents adopted. Nevertheless, the history of chemistry shows that for more than thirty years no changes have been judged necessary for well known bodies, and that those which have been admitted for elements, whose properties or whose combinations had previously been imperfectly known, were so thoroughly justified by their chemical properties, that even the equivalents of these bodies have had to be modified. Such was the case for bismuth, uranium, vanadium, tantalum and niobium. In reality, the only important change that atomic weights have had to suffer, since their introduction in chemical science, has been the reduction to half of the weights of silver and of the alkaline metals, a reduction based on their specific heat in the solid state; on the specific heat of their combinations, or on isomorphism as was done in the first instance by M. Regnault.* We may see by this that, on the score of invariability, the two systems are on a par.

As to the advantage which results from the fact that equivalents express ratios of real chemical equivalence, in cases where they are not indicated by atomic weights, it would be an important one if chemical equivalence were indicated in all cases; but we know that this is not so. It is really not more difficult to conceive and to remember that an atom of oxygen is worth two of chlorine, and an atom of lead two of silver than to know that an equivalent of nitrogen is worth three of oxygen, and that two equivalents of aluminium are worth three of magnesium. So there is really no advantage, on these two heads, which can counterbalance those which I have shown for atomic notations.

It may be said that the preceding is a contradiction of what I said before. I said that the system of equivalents presents conditions of invariability that are not presented by atomic weights. Further on I have shown that every change of atomic weight had necessitated a corresponding change in equivalents.

If we look for the cause of this apparent contradiction, it seems to me that we shall be led to make an observation which gives the key to the discussion actually going on. It is that, in reality, if we keep out of sight every question as to the *origin* of the terms *equivalents* and *atomic weights*, there is no difference between the two systems, and the partisans of equivalents are willing enough to accept the principles which serve to determine atomic weights, except when the necessity arises of changing the formulas of bodies that are of great importance and occur with great frequency.

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* Annales de Chimie et de Physique, 1841, III, vol. i, p. 191.

ART. XIV.—*Some thoughts on the Glycogenic function of the Liver and its relation to vital force and vital heat;* by JOSEPH LECONTE.

[Read before the National Academy of Sciences, New York, October, 1877.]

THE great size of the liver and its persistence as a conspicuous organ, as we go down the animal scale even to a very low position, clearly demonstrate the great importance of its functions. This conclusion is entirely confirmed by the very grave effects on the health produced by its disorders. But in spite of its acknowledged importance, great obscurity still hangs about the true nature of its functions. The function of the liver is not simple, like that of the lungs or the kidneys, but very complex. The liver is the manufactory of both bile and sugar. The bile is both a *secretion* used in the digestive preparation of food, and an *excretion*, separating poisonous matters from the blood. The sugar, too, has doubtless as many and as complex uses; but these are little understood. Evidently, therefore, any light, even the smallest, thrown upon this sugar-making function of the liver will be hailed with pleasure both by the scientific physiologist and by the medical practitioner.

What I have to offer on this subject, however, is not the result of any elaborate research, nor of the discovery of any new facts, but simply the result of thought on, and necessary deduction from, facts already known. Neither are all the thoughts and deductions entirely new, but I cannot find that they have been held sufficiently firmly and stated sufficiently clearly by physiological writers, and for this reason, perhaps, have not borne any fruit in medical practice.

It will not be amiss to review very briefly the main and usually acknowledged facts connected with this function.

1. The portal blood of *flesh-fed* animals contains no sugar, but the same blood, after passing through the liver, i. e., the blood of the hepatic vein, contains always a notable quantity of this substance. Evidently, therefore, *it is manufactured in the liver.*

2. If the dead liver removed from the body be washed out completely by water injected through the portal and hepatic veins until every trace of sugar is removed, and then the liver be allowed to stand a while, on recommencing the transmission of water the first that passes is decidedly sugary. The same process may be repeated several times with the same result, until the material out of which the sugar is made is finally exhausted.

3. If the liver of any animal be kept a considerable time before cooking, the amount of sugar which accumulates in its

substance is so large as to be easily detected by the taste. The liver is *decidedly sweet.* I have very often detected this sweetness. I have not seen attention drawn to the fact.

4. Evidently, therefore, the liver is constantly forming sugar from some *insoluble* or *feebly soluble* substance, which on account of its feeble solubility cannot be easily washed out. This substance has been isolated by Claude Bernard and others, and its properties determined. It is a white, feebly-soluble, tasteless, amorphous substance, having the composition of starch or dextrin. It changes easily and rapidly into sugar by mere contact of blood or other albuminoid ferment. It is called glycogen or sugar-maker. It is a kind of *animal starch.* The quantity of glycogen in the liver varies much with the food, being greatest (17 per cent) with amyloid food, and least (7 per cent or even only 2 per cent) with albuminoid food. (Pavy.)

5. The sugar formed from glycogen (liver-sugar) is closely allied, perhaps in composition identical, with glucose; but apparently differs from this and all other forms of sugar in being more unstable, i. e., more easily fermented and especially *more easily burned or oxidized in the blood.* It therefore probably differs from glucose in *molecular structure* if not in chemical composition. Sugar in any other form than that made in the liver, may circulate for some time in the blood without oxidation, but liver-sugar is speedily burned. The great importance of this difference is usually overlooked.

6. This change from glycogen into liver-sugar—this manufacture of sugar in the liver—is *not a vital but a purely chemical process.* It takes place in the *dead* just as well as, or even much better than, in the *living* liver.

Such are the undoubted *facts* in regard to the formation of sugar in the liver. But the physiological *uses* of this function have remained in a large measure at least, a mystery. There are, however, some other equally undoubted facts which are believed to throw light on this point, and which therefore we will briefly state.

1. The amount of amyloids which may be taken, and often is taken, as food, by a healthy man in the course of a day, is certainly one or two pounds. The whole of this is converted into sugar before it can be taken up. Two pounds of sugar, therefore, may be taken into the blood of a man who is fed largely on amyloid food.

2. This amount of sugar in the blood would make that liquid as sweet as syrup. But so far from this being the fact, sugar is an extremely minute, a scarcely detectable ingredient of the blood of the *general circulation;* so much so, indeed, that it is rarely put down among its normal constituents. What

then becomes of it? Burned up, it will be answered. Yes, it is doubtless burned up; but this combustion occupies at least twenty-four hours, and in the meantime a large though gradually decreasing quantity ought to be found in the blood unless it is disposed of in some other way. How is it disposed of?

3. The dissolved food is presented to the absorbents of the stomach and intestines in three principal forms, viz: as *peptones* (or dissolved albuminoids), *sugars* (dissolved amyloids) and *fats*. Of these the sugars are taken up mostly by the *capillaries* of the stomach and intestines (portal capillaries), the *peptones* partly by the capillaries and partly by the lacteals, and the fats wholly by the lacteals. Nearly the whole of the sugar and a large portion of the peptones, therefore, pass into the portal vein and thence by a second capillary circulation are distributed through the liver before they can enter the general circulation. The fats, on the contrary, with much of the peptones and some of the sugar are carried by the thoracic duct and discharged directly into the general circulation at the junction of the subclavian and jugular veins and thence are carried immediately to the heart to be distributed everywhere.

Now, put these three facts together: (1) the large amount of sugar taken up from the stomach and intestines of amyloid-fed animals, (2) nearly the whole of this sugar is distributed through the liver before it can reach the general circulation, (3) the minute proportion of sugar found in the general circulation, and the conclusion seems unavoidable, viz: *The whole of the sugar is arrested in the liver, changed into a less soluble substance nearly related to sugar, viz: glycogen, and thus withdrawn from circulation and stored in the liver. The stored amyloid is then slowly re-changed to sugar, but in the more oxidable form of liver-sugar*, and re-delivered, little by little, to the blood by the hepatic vein, as the necessities of combustion for animal heat and vital force require.

The views thus far presented have been held with more or less firmness by most physiologists since Bernard's discovery of this function. In what follows I have attempted only to push these views to what seems to me their legitimate conclusions.

It will be observed that there is here a double change in opposite directions. The liver seems to change sugar by dehydration into glycogen only to change by rehydration glycogen back again into sugar. At first sight there seems to be, therefore, a waste of force such as never occurs in the animal body. This has been urged by Pavy and others as an objection to this double change, as a fact. But there are at least two good reasons for this double change. First: *A large quantity of sugar* (glucose) *in the blood is very hurtful.* The experiments of Dr. Wier Mitchell, of Philadelphia, have proved that large quantities of sugar in the blood produce, among other hurtful effects, *cataract* and *blindness.* The cataract, so common among diabetic patients is doubtless due to this cause. Plainly, therefore, there is needed a *reservoir* to receive and detain the flood of glucose poured into the blood after every full meal of amyloids. The liver is that reservoir. A second reason for this double change—(and this is probably the fundamental reason)—is that the sugar is re-delivered to the blood not as glucose but as liver-sugar, and therefore *in a more oxidable form—as a better fuel* than previously.

For the sake of greater clearness, we have spoken thus far as if glycogen were made only from sugar. Most of it is undoubtedly made in this way in all animals except carnivora. But it seems certain that the *liver has the power of making glycogen from albuminoids also;* for animals fed on albuminoids alone still continue to make sugar from glycogen. Therefore—and this is a very important point—*albuminoids are decomposed in the liver into glycogen and some nitrogenous matter*, which is separated and excreted partly in the bile, but probably mostly restored to the blood to be excreted as urea by the kidneys. In this way the excess of albuminoid food, *over and above what is necessary for tissue-building is reduced to a condition suitable for easy combustion.*

But if this view be correct—if this be the way in which albuminoid *food* in excess of the requirements of tissue-building is disposed of, then it is certain that *waste tissues also* are eliminated in the same way, for these are also albuminoid. The first step in the elimination of these takes place in the liver where they are decomposed into glycogen to be burned as sugar and eliminated through the lungs, and a nitrogenous residue to be eliminated mostly by the kidneys. If so, then animals starved to death ought to make glycogen and liver-sugar to the last. Now, according to Chauveau, horses and dogs after six days fasting still continued to make liver-sugar.*

Again: we have spoken thus far only of the dissolved food taken up by the portal capillaries and distributed through the liver before entering the general circulation. By far the larger part undoubtedly takes this course; but a small portion also is taken up by the lacteals. This enters the general circulation through the thoracic duct, little by little, in proportion as it is taken up. But this also must pass, though not so directly, through the liver by the hepatic artery or by the mesenteric artery and portal vein and is doubtless also arrested there in the form of glycogen.

There are, then, *three sources* of glycogen, and therefore of liver-sugar, viz: 1. *The whole of the amyloid food.* 2. *All*

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* Comptes Rendus, vol. xliii, p. 1008.

albuminoid food in excess of what is required for tissue-building; and 3. *All waste tissues.* But since in the mature animal waste tissue is exactly balanced by tissue-building, the sum of the last two sources is exactly equal to the whole albuminoid food; and the material used for glycogen-making is, therefore, equivalent to the whole food, unless we except fats. The amyloid material is changed *without loss*, but the albuminoid material is changed with *large loss of nitrogenous residue*, which is eliminated partly as the nitrogenous matter of the bile, but mostly as urea by the kidneys. There are also the *same three sources of vital force and vital heat*, viz: 1. Combustion of amyloid food. 2. Combustion of the combustible portion of albuminoid food-excess. And 3. Combustion of the combustible portion of waste tissues. And, for the reason already assigned, this is exactly equivalent to combustion of the combustible portion of the whole food. Therefore, *the sole object of the glycogenic function of the liver is to prepare food and waste tissue for final elimination by lungs and kidneys; to prepare an easily combustible fuel, liver-sugar, for the generation of vital force and vital heat by combustion, and at the same time a residuum suitable for elimination as urea.* And conversely: *the only combustible used in the body is liver-sugar*, unless, indeed, we except fats, the mode of combustion of which is not known. We at once see, then, the importance of this function, lying, as it does at the very foundation of all the eliminative processes, as also of all force-making and heat-making processes of the animal body; and we are led to anticipate that the failure of this function must entirely sap the vitality of the system. That such is a fact we shall presently see.

There are several important conclusions to be drawn from the above to which I would next call attention.

1. The real function of the liver in this connection is not glycogenic or *sugar-making* as usually supposed, but glycogenic or *glycogen-making.* Glycogen-making is a true *vital function;* sugar-making is a pure *chemical process.* The former is an *ascensive*, the latter a *descensive*, metamorphosis. The former can take place only in the *living* liver: the latter also in the *dead* liver, and even more rapidly, because the opposite force is withdrawn and the contrary process of fixing is stopped. In this as in so many vital processes we may, in the present state of our knowledge, best represent the condition of things as the result of two opposing forces, a higher vital and a lower chemical; the former producing ascensive, the latter descensive changes. In death the latter triumph, and the changes are descensive only. These two opposite processes in the liver are fully recognized by Claude Bernard, although for convenience both processes are called glycogenic.

2. In the well-known and usually fatal disease, diabetes, sugar is excreted in large quantities by the kidneys. But the kidneys are not the organ in fault. On the contrary they do all they can to remedy the evil. Sugar in the blood is extremely hurtful; the kidneys remove it as fast as they can. Some have supposed that the lungs are in fault. The sugar contributed by the liver to the blood, they say, is not burned up as fast as received (as it ought to be) on account of deficiency of oxygen taken in by the lungs. But the true organ *directly* in fault is undoubtedly the liver. In diabetic patients the liver seems to have *lost its glycogen-making power.* The sugar taken up by the portal capillaries is not arrested in the liver, but allowed to pass straight through unchanged into the general circulation, whence it must, of course, be eliminated by the kidneys. The difficulty is not *too much sugar-making but failure to arrest the sugar as glycogen.* This is proved by cases of *traumatic* diabetes. It is well known that puncture of the floor of the fourth ventricle of the brain produces diabetes. In such cases, even though sugar be ingested in large quantities there is no glycogen formed in the liver; the glucose having been allowed (probably on account of paralysis of the vaso-motor nerves) to pass straight through unchanged into the general circulation. The extreme gravity of *pathological* diabetes, therefore, is partly the result of the directly hurtful effects of sugar in the blood, as shown by diabetic cataract, but chiefly owing to serious disorder of a very important function of the liver—a function on which wholly depends animal heat and animal force of all kinds, viz: the preparation of food and waste-tissue for easy combustion. The use of a pure albuminoid diet may remedy the former difficulty, i. e., the presence of excess of glucose in the blood, but cannot remedy the latter and greater difficulty. We at once see, therefore, why an extremely low vitality and a temperature which is subnormal, even in fever, should characterize this disease.

3. There is a very interesting analogy between this glycogen-making function of the liver and the *starch-making function of plants.* Plants change the soluble forms of amyloids, viz: dextrin and sugar into the insoluble form of starch. They do so for the purpose of storing it away for *future use.* When ready to use it they again reconvert it into the soluble form of dextrin or sugar and take it again into circulation. It cannot be *stored* unless in the *insoluble* form; it cannot be *used* unless in the *soluble* form. So animals also change soluble sugar into insoluble glycogen (animal starch) and store it away in the liver for future use. In order to use it, however, it must be reconverted into the soluble form of sugar.

In a work entitled "Digestion Végétale," published in 1876,

M. Morren likens the change of starch into sugar in plants to digestion in animals, and concludes that the function of digestion can no longer be regarded as peculiar to animals, but is universal among organisms. But, as we have already shown, the true analogue both of starch-formation and starch-solution in plants is found in the glycogenic function of the liver. Digestion is properly a solution of *solid food*, preparatory to its absorption into the circulation. No such function is needed in plants (unless we except insectivorous plants) because their food is already liquid. Materials already absorbed and stored in the tissues cannot be called food, nor can changes in such materials be called digestion except by a metaphor.

4. The analogy between animals and plants as regards this function of storing amyloids is far more striking in the lower animals and in embryonic conditions. All parts of plants seem to have the power of fixing and storing amyloids, although it is mostly confined to parenchyma or undifferentiated cellular tissue; while in the higher animals this function is confined to the liver. But it is an interesting fact showing the greater resemblance of lower animals and of the embryonic conditions of higher animals, to plants, and illustrating the law of differentiation both in the development of the embryo and in the evolution of the organic kingdom, that the embryos of the higher animals and the adults of many lower animals, have also the power of fixing amyloids or making glycogen, *in all parts*, although this is especially true of undifferentiated *epithelial tissue* (Bernard.) And, what is very noteworthy, it is *sluggish* animals, or animals least endowed with nervous energy, which accumulate most glycogen in their tissues. Bizio found 10–14 per cent in the *dried* tissues of the oyster and cardium. Foster fully as much or more in entozoa, viz: two per cent wet weight. Thus as we pass up the animal scale not only do we find this function more and more localized in one organ, the liver; but also the glycogen product more and more quickly consumed by combustion for animal activity.

5. But if there are striking resemblances, so also are there differences in the use of stored amyloids very characteristic of the two kingdoms. Plants store away starch for future use as *building material:* of course, therefore, after re conversion into sugar it must be again re-converted into insoluble cellulose. Animals on the contrary store away glycogen for future use as *fuel for force-making.* Plants do not require it for force for *they draw their force from the sun.* Animals do not require it for tissue-building *for their tissues are wholly albuminoid.*

6. We have seen that albuminoids, whether food or waste tissues, are probably split in the liver into glycogen and some nitrogenous residuum. The glycogen is changed into sugar and

then by oxidation into CO_2 and H_2O and eliminated by the lungs. The nitrogenous residuum if it is not at first urea is at least easily changed into urea and eliminated by the kidneys. We see then the *close relation* between the functions of the liver and kidneys. Now in going down the animal scale, we find in many cases, as for example in insects, *that the same organ performs both functions.* In the process of evolution urea was at first formed and excreted at once by the same organ. Afterwards these two processes were differentiated.

7. We have already stated that Foster found a large percentage of glycogen in the tissues of entozoa.* He asks, "Of what use is this glycogen?" and answers, "No respiratory use" is possible, for "having a constant temperature secured to them" by the body of their host, "they are the very last creatures to need respiratory or calorifacient material." He, therefore, inclines to the view of Pavy, that glycogen is not respiratory material at all, but material on its way to become fibrin. On the contrary, *we*, for our part, see in this observation of the presence of stored amyloid in an animal not needing any internal source of heat, a striking proof (if any were still needed) that the prime object of respiration is not *heat-making but force-making;* and the view of Foster we regard as a no less striking proof of the fact that this fundamental principle of physiology is even yet imperfectly recognized by some of the best men. The very use of the term heat-making material as synonymous with respiratory material shows a profound misconception. But this expression as applied to amyloids and fats has unfortunately become so universal, and is now so hedged about by authority of great names that it is almost impossible to eradicate the false ideas associated with it. It cannot be too often or too strongly enforced that the prime object of combustion in the animal machine, as in the steam engine, is not heat-making but *force-making.* Heat is only a *concomitant*, often useful, but sometimes useless and even distressing. Careful experiments have over and over proved this; the doctrine of conservation of force absolutely requires it; and yet physiologists still go on serenely talking of respiratory food as only heat-making, as if vital force were something wholly unrelated to other forces of nature and therefore requiring no explanation at all. The vital force created by combustion may be expended largely in mechanical power, as in the higher animals, or almost wholly in the vegetative functions of digestion, assimilation, secretion, &c., as in the more sluggish lower animals such as many mollusca and entozoa.

Postscript.—Since finishing this article in April last, I have read with deep interest a very remarkable paper by Professor

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* Proc. Roy. Soc., xiv, 543.

Schiff,* entitled "A new function of the liver," in which the author demonstrates in the most convincing manner by means of experiments on dogs and frogs conducted in the physiological laboratory in Geneva, that *the liver has the power of completely decomposing poisonous matters generated by the wasting of tissues.* Ligation of the vessels of the liver, especially of the portal vein, in mammals, quickly produced extreme lethargy and finally death in the course of one to three hours. In frogs, it is true, ligation produced little effect, but this is only because of the slowness of the changes in their tissues; for the blood of dogs dead of ligated livers injected into the circulation of frogs quickly destroyed life if their livers are ligated, but produced no effect if the livers are unligated. Moreover many other organic poisons were shown to be either wholly or partly destroyed and rendered innocuous by the liver. Doubtless this is the true explanation of the more potent effect of organic poisons when administered by subcutaneous injection; for they then enter the general circulation without passing through the liver.

I regard these experiments as a most striking confirmation of the view presented in this paper, viz: that waste tissues are decomposed in the liver into glycogen and a nitrogenous residuum eliminated mostly by the kidneys. But if my view as to the mode of decomposition be correct, then the experiments of Professor Schiff have demonstrated not a *new function*, but rather an extension of the glycogenic function, throwing upon it a *new light* and giving it a *new significance.* It is precisely this new significance which I have attempted to set forth in the foregoing paper.

Berkeley, Cal., Oct. 15, 1877.

ART. XV.—*Revision of the Atomic Weight of Antimony;* by JOSIAH P. COOKE, Jr.

(Concluded from page 49.)

AFTER a long series of experimeuts, which threw but little light on the cause of the discrepancy that had now become so prominent, although they yielded very interesting subsidiary results which are described in detail in the original paper, we made a series of analyses of antimonious bromide with the hope that if there was, as we suspected, a source of constant error in the analyses of anitimonious chloride, the same influence would affect the analyses of the corresponding bromide to a different degree and might thus reveal its nature.

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* Arch. des Sciences, lviii, 293, March, 1877.

We prepared the bromide of antimony by adding in small portions at a time the pulverized metal to a strong solution of bromine in sulphide of carbon. The retort containing the solution was kept cool by snow, and shaken after each addition until the action ceased. As soon as the color of bromine was discharged, the sulphide of carbon was distilled off over a water bath; and then, replacing the water bath with a gas lamp, the bromide of antimony was first boiled, and then distilled over the finely powdered antimony which had been added in excess. On account of the high boiling point of bromide of antimony, and the readiness with which its vapor condenses, it was found best in distilling to cover the body of the retort with a hood. The bromide thus prepared was purified by repeated distillations over pulverized antimony, as in the case of the chloride, and finally by crystallizing and recrystallizing several times from solution in purified sulphide of carbon. A warm saturated solution in sulphide of carbon deposits, when cooled to the freezing point, the greater part of the bromide of antimony in fine acicular crystals. These crystals were dried first with blotting-paper, and then *in vacuo* over sulphuric acid. The antimonious bromide thus purified by fractional distillation and crystallization was only a very small fraction of the first crude product. It was pure white, had a high silky luster, and, when first made, was wholly destitute of odor. It was carefully examined for chlorine, iodine, and arsenic; but the delicate tests which we possess for all three of these elements so frequently associated with commercial antimony and bromine, failed to show the least trace of either in the bromide of antimony we analyzed. The determinations of bromine were made in all respects like those of chlorine. Great care was taken not to add more than a very slight excess of argentic nitrate, and we found that under these conditions the supernatant liquid cleared more readily above the precipitate in the case of bromide of silver than with the corresponding chloride, and for this reason the first could be washed more quickly than the last. The results of these determinations are embodied in the table on the following page.

Here, as before, the letters indicate different preparations: *a* was made and purified as described above; *b* was the same material as *a* redistilled and again crystallized from bisulphide of carbon; *c* was another portion of the same material several times redistilled and twice recrystallized from the same solvent; *d* was a separate preparation from the start; *e* was another separate preparation purified with extreme care. In the last case there was over a kilogram of the crude product, which was reduced by the fractional distillation and crystallization—each process repeated from ten to twenty times—

to the few grams used in the analyses. These methods of purifying the substance were thus pushed to their utmost limits.

ANALYSES OF ANTIMONIOUS BROMIDE.

DETERMINATION OF BROMINE.

No.	Wt. of $SbBr_3$ taken in grams.	Wt. of AgBr obtained.	% of Bromine Br=80, Ag 108.
1, *a*.	1·8621	2·9216	66·765
2, *a*.	0·9856	1·5422	66·584
3, *b*.	1·8650	2·9268	66·779
4, *b*.	1·5330	2·4030	66·703
5, *b*.	1·3689	2·1445	66·663
6, *c*.	1·2124	1·8991	66·655
7, *c*.	0·9417	1·4749	66·647
8, *d*.	2·5404	3·9755	66·593
9, *d*.	1·5269	2·3905	66·623
10, *e*.	1·8604	2·9180	66·743
11, *e*.	1·7298	2·7083	66·624
12, *e*.	3·2838	5·1398	66·604
13, *e*.	2·3589	3·6959	66·671
14, *e*.	1·3323	2·0863	66·635
15, *e*.	2·6974	4·2285	66·708

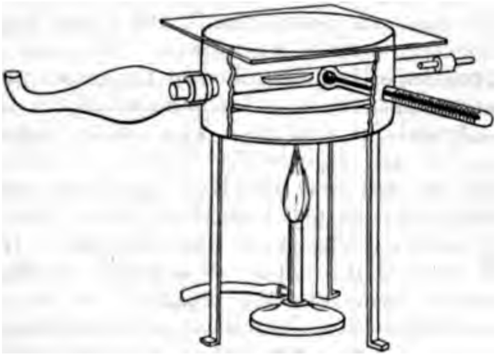
Mean value from last six determinations, 66·664
Mean value from all the determinations, .. 66·6665
Theory Sb 120 requires, ... 66·6666
Theory Sb 122 " .... 66·2983

If in calculating the result of the above bromine determinations we use the atomic weights of Stas,—Br=79·952, Ag=107·93,— the per cents found will be in each case only 0·002 higher, which is, of course, an inappreciable difference. Hence, whether we take Stas's or Dumas's values for the atomic weights of bromine and silver, the atomic weight of antimony deduced from the above determinations is exactly 120·00.

This is certainly a remarkably close confirmation of our previous conclusion. Indeed the wonderful coincidence between the observed and the theoretical results must be to a certain extent accidental; for no process of chemical analysis is capable of the accuracy which this agreement would imply. Still it should be noticed that the probable errors of the process, so far as they are indicated by the variations from the mean value, are not larger than we might expect would be eliminated by multiplying observations; and, further, that the mean of the last six determinations which are undoubtedly the most trustworthy, is nearly as close to the theory as the mean of the whole.

But not only did these experiments on bromide of antimony thus confirm our previous conclusion: they also gave the first definite clew to the explanation of the disagreement with otherwise consistent results which our experiments on chloride of antimony had presented. The one difference between the chloride and the bromide, which appeared to render the last better suited to yield accurate results, was the difference in their hygroscopic qualities. As we have stated, the chloride is one of the most hygroscopic substances known. The bromide is also hygroscopic, but far less so, presenting no unusual difficulties of manipulation; and, since our tests indicated that both substances were otherwise pure, we at once drew the inference that the different results we had obtained with chloride of antimony must depend on the extraordinary attraction of this substance for moisture. Before, however, fully following out the clew thus obtained, we made a similar study of the iodide of antimony.

The iodide of antimony was prepared like the bromide, by shaking up in a glass flask a solution of iodine in bisulphide of carbon with finely pulverized metallic antimony. On filtering and decanting, after the color of the iodine is discharged, a solution having a pale greenish-yellow color is obtained, from which on cooling or on evaporation red crystals of iodide of antimony are deposited. The substance may be purified by recrystallization from the same solvent; but iodide of antimony is far less soluble in bisulphide of carbon than the chloride or bromide, and cannot therefore be so advantageously treated in this way, nor can the small amount of carbonaceous impurity which the crystals acquire from the solvent be so easily removed. Moreover, iodide of antimony cannot be so readily distilled as the chloride or bromide, on account of its high boiling point, which is above that of metallic mercury. But another property of iodide of antimony which, so far as we know, has not hitherto been noticed, interferes still more seriously with these methods of purifying this substance. In all its conditions, it undergoes a more or less rapid oxidation in contact with atmospheric air, forming oxi-iodide of antimony (SbOI) and free iodine. When iodide of antimony is rapidly boiled in a small flask, so that the body and most of the neck are kept full of vapor at the boiling-point, the action at the surface of contact of the vapor and the air is very striking; iodine is set free in vapor, with its familiar violet color, while the oxi-iodide is precipitated in clouds, forming a most beautiful phenomenon. So also when the greenish-yellow solution (above described) of the iodide in bisulphide of carbon is exposed to the air and light, it rapidly becomes colored red from the liberation of iodine, and at the same time turbid from the osition of the insoluble oxi-iodide. Even the crystals of ide of antimony, when kept in the light, slowly become que from the formation of the same oxi-iodide; while the r and staining of the stopper of the bottle furnish abundant of of the liberation of iodine. The study of these phenena was most interesting, and the results obtained will be cribed in another paper. It is sufficient for the present to that they pointed out to us a great source of impurity in ide of antimony, and fully explained the want of accordance our analyses of the crystals of this substance as first pre- ed. It was evident that we could only hope to purify the terial by distilling or subliming it in an atmosphere of rt gas; and we devised the apparatus represented in the acnpanying figure for this purpose, which we have since found y generally useful for all sublimations where the temperature uired does not exceed that which can be measured with a rcury thermometer. The apparatus has been already refer- to, and requires no further description. It was a simple dification of the apparatus used before for drying at a regud temperature the precipitates of sulphide of antimony, ich, as we have stated, was so arranged that the character of sublimates which might be given off could be observed. used the same glass tube passing through the sheet-iron bath, with its transparent mica cover, only we added a comn glass adapter, selected so that its mouth just fitted over open end of the tube. A platinum nacelle containing ide of antimony, which had already been purified by crysization, was placed in the tube within the air-bath, but near open mouth; and, while a current of dry carbonic dioxide ough the apparatus was steadily maintained, the air-bath heated by a gas lamp to the required temperature which indicated by a thermometer as shown in our figure. Iodide antimony is sensibly volatile, even at 100°; and long before



it reaches its melting point, 167°, the evaporation becomes very marked. As soon as melted, it sublimes quite rapidly; and we obtained the best results by keeping the temperature between 180° and 200°, and by shifting the adapters we used as receivers, it was easy to collect the different portions of the sublimate. We thus obtained crystals of two isomeric modifications of iodide of antimony: the more abundant in large hexagonal plates, often half an inch or more in diameter, perfectly transparent, and of the most brilliant ruby-red color; the other in small rhombic plates, having the same peculiar greenish-yellow color as the solution of the iodide already mentioned. The amount of the last was always small, but it was larger in proportion as the temperature was lower. This new and most interesting product will be described in the paper just referred to. Of these crystals, the most brilliant, chiefly of the red variety, were selected for analysis. The iodine determinations were conducted in all respects like those of chlorine and bromine. The iodide was first dissolved by a very concentrated solution of tartaric acid, and then the solution was diluted to the required extent. The same care was taken not to add more than a very slight excess of argentic nitrate, and the amount required was accurately weighed out in each case. Each of the determinations was made with a separate preparation in so far as it was a product of a separate sublimation; but the material sublimed was essentially the same in all cases, —a mixture of the products of many crystallizations from the crude material made as described above. The results are collected in the following table:

ANALYSIS OF IODIDE OF ANTIMONY.

IODINE DETERMINATIONS.

No.	Wt. of SbI_3, grams.	Wt. of AgI, grams.	% of Iodine, I=127, Ag=108.	Variety.
1.	1·1877	1·6727	76·110	Pure red.
2.	0·4610	0·6497	76·161	Chiefly yellow.
3.	3·2527	4·5716	75·956	Pure red.
4.	1·8068	2·5389	75·939	Pure red.
5.	1·5970	2·2456	75·990	Red and yellow.
6.	2·3201	3·2645	76·040	Pure red.
7.	0·3496	0·4927	76·161	Chiefly yellow.

Mean value........................76·051
Theory Sb=120, requires............ 76·047
Theory Sb=122, " ....75·744

If in calculating the results of these iodine determinations we use the atomic weights of Stas, I=126·85 and Ag=107·93, the mean value would be 76·034, and the corresponding atomic weight of antimony 119·95.

The difference (0·004) between the first mean value and theory—corresponding to only about $\frac{3}{10}$ of a milligram in the largest amount of argentic iodide weighed—is evidently insignificant, so that these results confirm the lower value of the atomic weight of antimony as closely as did the analyses of the bromide.

As we have already intimated, our analyses of the iodide of antimony, as first crystallized from bisulphide of carbon, yielded very discordant results. These we give in the table below, not, as before, in the exact order in which the analyses were made, but in the order of the several values, so as to exhibit the distribution of the errors.

ANALYSES OF CRYSTALLIZED ANTIMONIOUS IODIDE, RED VARIETY.

No.	% of Iodine.
1	75·71
2	75·76
3	75·78
4	75·80
5	75·84
6	75·85
7	75·87
8	75·89
9	75·94
Mean value	75·83
Theory Sb=122	75·74
Theory Sb=120	76·05

The cause of this discordance we attributed, as we have intimated, chiefly to the remarkable readiness with which iodide of antimony undergoes oxidation in contact with the air, resulting in the formation of oxi-iodide of antimony and free iodine, thus:—

$$2SbI_3 + O\text{-}O = 2SbOI + 2I\text{-}I.$$

While the free iodine escapes, the oxi-iodide remains as an impurity in the preparation, and the effect is a replacement of a portion of its iodine by oxygen. Now, since eight parts of oxygen replace one hundred and twenty-seven parts of iodine, it can readily be seen that an otherwise almost imperceptible amount of oxidation would be sufficient to produce all the variation from the normal composition which the above results present. A simple calculation will show that an absorption of only $\frac{47}{1000}$ths of one per cent of oxygen, or less than half a milligram by each gram of iodide of antimony, would reduce the per cent of iodine from the theoretical value, 76·047, to the mean of the above results, 75·832; and that a corresponding absorption of three-quarters of a milligram would reduce the per cent to 75·700, the lowest observed. It is not, therefore, surprising that we could obtain concordant results only with material which had been both purified by crystallization and also *recently* sublimed.

Returning now to discuss again the cause of the disagreement of the analyses of antimonious chloride with our otherwise consistent results in regard to the atomic weight of antimony, it was obvious that the strong hygroscopic power of the chloride must lead to a replacement precisely similar to that which is produced in the iodide by direct oxidation; for, as we have before said, the crystals of antimonious chloride cannot be exposed to the atmosphere for an instant without absorbing a perceptible amount of moisture, and every molecule of water thus absorbed reacts on a molecule of the chloride, thus:—

$$SbCl_3 + H_2O = SbOCl + 2HCl.$$

And when the antimonious chloride is boiled, the hydrochloric acid formed is given off, while the oxichloride remains behind, dissolved in the great mass of the liquid. Indeed, it seems impossible, with our ordinary appliances, to prepare or purify antimonious chloride without its becoming contaminated with oxichloride; and our experiments would indicate that when once it has been formed, as above described, in the mass of the material, it cannot be wholly removed by distillation or crystallization, however often these processes may be repeated.

Naturally, our attention was very early called to this obvious source of impurity in the antimonious chloride we prepared: and we noticed from the first that, even after the material had been many times distilled, there was always left, on repeating the process, a very small amount of dark-colored residue. We had examined the residue, and found that it was a mixture of chloride and oxichloride of antimony, colored by a trace of carbonaceous material; and we had made a long series of analyses for the purpose of studying the effect produced by the action we have described. The result of these analyses is given in the following table. We started with material already purified by fractional distillation and crystallization, and distilled it ten times in succession; not, however, carrying the distillation to absolute dryness, but leaving, so far as we could judge by the eye, about the same amount of residue in the retort each time. These residues we analyzed, as we did also the final distillate. The material first distilled was the same as that marked *c* in the table on page 47, and we assumed that the average of the results there given truly represented its composition.

ANALYSES OF ANTIMONIOUS CHLORIDE.

RESIDUES AND DISTILLATES.

	% of Chlorine.
original purified preparation	46·64
residue of 1st distillate	45·71
" 2d "	45·66
" 3d "	46·03
" 4th "	46·26
" 5th "	46·26
" 6th "	46·00
" 7th "	46·03
" 8th "	45·94
" 9th "	45·65
" 10th "	45·99
last distillate	46·62

lthough, under the circumstances, we could not expect t precision, yet it was evident from these analyses that the unt of impurity in the residues was not diminished by the essive distillations; and we therefore concluded that additi il oxichloride of antimony must be formed each time dur- the very short contact with the atmosphere which the ısfers between the several distillations necessarily involved. on the other hand, the very remarkable fact that these ten llations produced no sensible change in the composition of great mass of the material seemed to indicate equally clearly this action of the atmosphere had no perceptible influence he final result; and this opinion was still further strength- when, on twice distilling portions of the last distillate, at v temperature, in a current of dry hydrogen, we obtained lucts given again—very nearly at least—the same per cent ılorine. And, lastly, when to all this evidence were added results of the complete analysis of the chloride, showing an unt of antimony which fully supplemented the very con- t per cent of chlorine, the assumption that any material unt of impurity could be present appeared wholly unten- . Yet we have seen how this assumption was forced back n us by the subsequent results of the investigation.

eturning to the subject after our experiments with iodide ntimony, we, for the first time, fully appreciated how very ll an amount of oxygen—the only real impurity present— required to reduce the per cent of chlorine in antimonious ride from 47·02, the amount corresponding to Sb=120, to ı1, which corresponds to Sb=122; for, while the effect is so ırently produced, yet the result of the action of the atmos- re on the chloride of antimony is wholly like that of its on on the iodide. It ends in replacing a small amount of rine by oxygen; and although, in consequence of the smaller atomic weight of chlorine, it requires in this last case a larger replacement to produce a corresponding change of percentage composition, yet still the amount required to make all the difference in question is very small; so that, when we come to sum up the supposed completed results (as on page 47), it might easily be covered up by slight inaccuracies of the analytical work. An easy calculation will show that the substitution of but $\frac{146}{1000}$ of one per cent of oxygen for the equivalent amount of chlorine would reduce the per cent of this last element in the chloride from 47·020, corresponding to Sb=120, to 46·608, which corresponds to Sb=122; and such a substitution would result from the absorption of only $1\frac{6}{10}$ milligram of water by each gram of the chloride. The composition of the material would then be as follows:—

COMPOSITION OF ANTIMONIOUS CHLORIDE WITH $\frac{146}{1000}$ PER CENT OF O WHEN CL=35·5 AND SB=120.

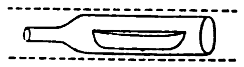
Chlorine	**46·608**
Oxygen	**·146**
Antimony	**53·246**
	100·000

Now it will be seen by referring to the tables, on pages 47 and 48, that these percentages do not differ from the mean of the results of our previous analyses as much as these results differ among themselves; and we therefore determined to repeat these analyses, hoping that the experience we had acquired in both chlorine and antimony determinations would now enable us to obtain results sufficiently sharp to show even the small differences of composition which the substitution in question would produce.

Meanwhile, we instituted a series of experiments with a view of studying the decomposition which the oxichloride of antimony undergoes under the action of heat, in the hope that we might thus discover some method by which the amount of oxichloride of antimony in our preparations might be directly determined. For this purpose, we used first crystallized SbOCl, obtained by the action of alcohol on chloride of antimony in a sealed tube, which we weighed out into a platinum nacelle, and heated to various regulated temperatures, using for this purpose the apparatus already described. It appeared that the decomposition took place in two stages. The first stage of the decomposition began between 167° and 175°, but was not completed until between 260° and 280°. The second stage began at about 320°, but required for its completion a red heat. During both stages, chloride of antimony sublimed; and there was left in the nacelle at the close of the process beautiful crystals of Sb_2O_3. In another experiment, we used crystallized $Sb_4O_5Cl_2$, prepared in the same way as the SbOCl, but with different proportions of alcohol and chloride of antimony. In this case, the decomposition did not begin until 320°, but in other respects both the process and the products were as in the first experiment. It was quite evident that the chemical changes which took place in the two stages of decomposition we have noticed were represented by the following reactions:

First stage: $5SbOCl = Sb_4O_5Cl_2 + SbCl_3$; (1)
Second stage: $3Sb_4O_5Cl_2 = 5Sb_2O_3 + 2SbCl_3$; (2)

but the relative weights observed in the first two experiments were of no value, because it was evident that a no inconsiderable amount of Sb_2O_3 was lost by sublimation. Since, however, the small sublimate of oxide condensed in the glass combustion-tube very much nearer the nacelle than the very much larger sublimate of chloride, we varied the apparatus in our third experiment so far as to place the nacelle in a tube of the shape represented in the accompanying figure. This tube was weighed with the nacelle, and was so selected that it quite closely fitted the combustion-tube within which it was placed for heating, as shown in figure by dotted lines. And it is evident that, while with this arrangement the $SbCl_3$ would be swept by the CO_2 gas into the colder portion of the combustion-tube, the greater part at least of the sublimed oxide would be retained in the small tube, which was of course at each stage weighed with the nacelle, as at first. Our results were as follows:—



Weight of SbOCl	0·4939	grams.
Loss at 280°	0·1271	"
Required by theory of reaction 1, if Sb=120	0·1305	"
Total loss at red heat; that is, in both stages	0·2179	"
Required by theory of reactions 1 and 2	0·2174	"

It was evident from this determination that the order of the decomposition was precisely that indicated by our reactions, although the end of the first stage was not quite so sharply marked as the end of the second; and this would naturally be expected.

As the residues obtained on distilling chloride of antimony showed, when further heated, precisely the same order of phenomena which we have just described, and when heated to redness yielded the same crystals of oxide of antimony as before, it was plain that the residue left on evaporating the chloride at a temperature not exceeding 120° was chiefly at least SbOCl; but that this when heated more intensely was converted into

$Sb_4O_5Cl_2$ before the temperature reached 280°, and finally at a red heat was converted wholly into Sb_2O_3. We therefore endeavored to determine the amount of oxichloride in one of our preparations of chloride of antimony by distilling a weighed amount from a platinum nacelle at as low a temperature as possible in a current of dry carbonic acid, and heating the residue to a temperature of about 275°. We thus obtained the following results:—

No.	Wt. of $SbCl_3$.	Residue.	% of residue $Sb_4O_5Cl_2$.
1.	6·7286	0·0212	0·315
2.	4·5150	0·0151	0·334
3.	7·9320	0·0258	0·325

In order to yield 0·146 per cent of oxygen, which would reduce the per cent of chlorine in the preparation from 47·020 to 46·608, as in the scheme on page 116, there would be required 1·155 per cent of $Sb_4O_5Cl_2$.

Although the results of the above determinations accord within a few per cent of the quantity estimated, yet it was perfectly clear during the course of the experiments that they did not at all represent the total quantity of the oxichloride present in the preparation examined. Not only was the composition of the preparation not materially altered by the slow distillation,—a fact shown by the determinations marked *e* in the table on page 47, and by which we were misled at the outset,—but also the product from our distillation yielded when distilled again apparently as much residue as before. In a word, we found the same phenomena repeated in these distillations at a low temperature which had been so noticeable when the chloride was distilled at its boiling point, and which are so strikingly illustrated by the results given on page 115. It is possible, as before suggested, that the effects might arise from a small additional absorption of water at the successive transfers which the repeated distillations involved; or, in the later experiments, from the circumstance that the very extensive apparatus employed for drying the carbonic dioxide was not completely effectual. Still, now that our attention has been called to the danger, and we had taken unusual precautions on both these points, the explanations suggested did not seem to us sufficient; and we came to the conclusion that the oxichloride must distil over with the chloride of antimony to a certain limited extent, and that it was only an excess above this definite amount which was left behind as residue. Of course, SbOCl not only is not volatile, but is at once decomposed by heat; and we do not suppose that this compound by the tension of its own vapor is carried over in distillation. It is a very dilute solution, as it were, of SbOCl in $SbCl_3$ which thus distils; and the distillation of the oxichloride may resemble the carrying over of boracic acid by the vapor of water, and similar phenomena, the result, as it is has always appeared to us, of a feeble kind of chemical union which has been usually designated by the term "molecular combination." Such a theory would account for the remarkable constancy which we have found in the chlorine determinations of the various preparations of antimonious chloride purified by distillation. But, on account of the very great difficulty of removing all possible disturbing causes, we found it impossible to obtain a rigid experimental demonstration of our theory without much more time and labor than we could then command. We hope to return to the subject hereafter. Meanwhile, however, it was evident that we could place no reliance whatever on the results just obtained. Nevertheless, the determinations were of value on account of the contrast between these results and those of a similar series of experiments on the residues from antimonious bromide which we collect in the following table:—

No.	Wt. of $SbBr_3$.	Residue chiefly $Sb_4O_5Br_2$.	% of residue.
1.	2·8342	0·0010	0·035
2.	2·0220	0·0006	0·030
3.	4·6730	0·0010	0·021

As will be seen, this residue is less than one-tenth of that obtained from the chloride, and is practically insignificant. Evidently, then, in the determination of the atomic weight of antimony more accurate results may be expected from the analysis of the bromide than from the analysis of either the chloride or the iodide of this element. The intermediate position of the bromide renders it, in a very remarkable way, the most stable of the three compounds. It absorbs moisture far less eagerly than the chloride, and it absorbs oxygen far less readily than the iodide, and is thus in great measure protected against each of these two sources of the same impurity.

We come finally to the new analyses of antimonious chloride we had undertaken. Fortunately, some of the old preparation that had been distilled so often had been preserved. It had been boiled for a long time since the last analyses were made, and kept in the same flask used for determining its boiling point, which had stood meanwhile tightly corked in a desiccator over sulphuric acid. The solid mass in the flask was easily broken up without exposure to the air by simply heating it to the melting point, and shaking it in the flask as soon as, beginning to melt; the mass had separated from the glass. Near its melting point, chloride of antimony becomes very friable, and is thus easily reduced to coarse powder, whence probably the old alchemistic name of butter of antimony. It is also worthy of notice that neither the bromide nor the iodide acts in this way, as we found out in more than one instance to our cost.

Thus we were readily able to prepare our material for analysis, and, by a thorough mixing of the mass, to insure that the several samples taken had a uniform composition. In regard to the antimony determination, no further details are necessary. It was conducted, as described before, with every minute precaution which experience had suggested; and we give the full details, in order to show how completely we had been able to overcome the difficulties which it at first presented, and we feel confident that there is no process of wet analysis which is capable of giving more accurate results than this.

DETAILS OF ANTIMONY DETERMINATION.

The antimonious chloride was first transferred to a very carefully dried weighing tube, and thence to the large flask in which it was dissolved. The transfer to the weighing tube was made in a dry atmosphere, and only required two or three seconds. It is evident, however, that a slight absorption of moisture at this point is not important; for, even if it increased the apparent weight of the assay by several milligrams, it would only reduce to a barely perceptible extent the percentages of all the constituents leaving the relative values wholly unchanged. It is only when, on boiling the chloride, after such an absorption, the chlorine is driven off, that the essential change of composition results.

Weight of tube and antimonious chloride	20·9609	grams.
“ “ after transfer to flask	16·3920	“
“ chloride analyzed	4·5689	“

The weight of the tube and chloride while on the balance pan remained invariable for a sufficient length of time to give positive assurance of the constancy of the weights. The chloride was dissolved in a saturated solution of tartaric acid containing about 15 grams of the pure acid, and then diluted with carbonic acid and water and precipitated as before described. The precipitate, having been washed and collected as before, was dried in an air bath, at about 110°.

Weight of small filter	0·0434	grams.
“ porcelain crucible	101·2132	“
	101·2566	“
“ crucible and precipitate	104·6762	“
“ red sulphide of antimony	3·4196	“

A portion of the dried precipitate dissolved in hydrochloric acid gave no residue. The rest was then transferred to a platinum nacelle, and heated, as has been described, in a current of dry carbonic dioxide gas. No sublimate was formed, and only a very slight empyreumatic odor could be perceived.

Weight of platinum nacelle	6·2493	grams.
" nacelle and dried precipitate	9·5273	"
" portion taken	3·2780	"
" nacelle and precipitate after heating to 285° for over half an hour	9·5234	"
Loss of weight of portion taken	0·0039	"
Corresponding loss for whole precipitate	0·0041	"
Weight of red sulphide as above	3·4196	"
" gray sulphide	3·4155	"

The carbonaceous residue left on dissolving this whole amount of gray sulphide in hydrochloric acid was barely perceptible. It was collected, however, as usual, on a weighed paper disk, and estimated.

Weight of small paper filter	0·0198	grams.
" same with residue	0·0212	"
" residue	0·0014	"
Calculated for whole precipitate	0·0015	"
Weight of gray sulphide as above	3·4155	"
Total weight of gray sulphide	3·4140	"
Corresponding weight of antimony assumed to be $\frac{5}{7}$ of the sulphide	2·4386	"
Per cent of antimony in the antimonious chloride under examination	53·374	"

It will be noticed that this result is practically identical with the mean of the previous determinations, which, as will be seen by reference to the table on page 48, was 53·401; and, by reviewing the facts stated in that connection, it will be perceived that this agreement is in itself alone a strong confirmation of the conclusion which we deduced from our first experiments on the synthesis of the gray sulphide of antimony,—that of the two values of the atomic weight of antimony in question, the lower is the more exact.

Coming next to the chlorine determinations, we noticed, for the first time, an effect which, under certain circumstances, may have an important influence on the accuracy of this well-known process, as employed in the analysis of chloride of antimony. In a precipitate of argentic chloride that had been deposited from an unusually concentrated solution of antimonious chloride in tartaric acid, and had stood over night, our attention was called to some crystalline grains, which, on examination, proved to be a compound of tartaric acid, antimony and silver. We soon found that this product could be readily obtained by concentrating the filtrate from the precipitate of argentic chloride, and adding to it, while still warm, an excess of argentic nitrate. On cooling, the new crystals form in abundance. They have not yet been measured, but under the microscope they have the general aspect of right rhombic plates or prisms, with hemihedral modifications,—a general form which is so characteristic of the tartrates, and which we ourselves have previously studied in our crystallographic determinations of the tartrates of rubidium and cæsium.* We obtained for the amount of silver in the crystals, as a mean of three analyses, 26·30 per cent. The compound $\mathrm{Ag, SbO.H_2{=}O_4{=}(C_4H_2O_2).H_2O}$ would require 26·34 per cent. The crystals may therefore be regarded as tartar emetic, in which the potassium has been replaced by silver; and they resemble the crystals of this well-known salt in general form. They are evidently the same substance obtained by Wallquist† by precipitating nitrate of silver with tartar. emetic, and analyzed both by him and by Dumas and Piria. These chemists obtained respectively 27·31 and 28·05 per cent. of oxide of silver, which corresponds with the result given above as closely as could be expected; but they appear to have prepared the substance only in an amorphous condition. At least, in the description quoted, no mention is made of any crystalline form.

These crystals of argento-antimonious tartrate are apparently not acted upon in the least by cold water, and only slightly by boiling water; and finding this very insoluble material mixed with the precipitated chloride of silver, under the conditions stated, we were led to fear that it might be occluded to some extent by this precipitate, even when formed in much more dilute solutions of antimony and tartaric acid. The phenomenon was very similar to that we had already studied in the occlusion of the oxichloride by the sulphide of antimony; and there was reason to fear that, as in the previous case, an occlusion of this double tartrate might result, even when the substance would not otherwise be precipitated. How far such an action could have vitiated our previous results, it was, of course, now impossible to determine; but, as we previously stated, we had always taken great care not to add more than the slightest possible excess of argentic nitrate, and this was especially true in our more recent determinations. Now, however, we were on our guard, and in the following determinations very great pains were taken to add just the requisite amount of the silver salt, and the argentic chloride was subsequently examined for traces of any such occlusion. But, excepting this close attention to well-known precautions, the determinations were made in the same way as before.

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* Am. Jour. of Science and Arts, II, xxxvii, 70.

† Gmelin Handbook, Cavendish Edition, x, 326,

ANALYSIS OF ANTIMONIOUS CHLORIDE.

No.	Wt. of $SbCl_3$.	Wt. of AgCl.	% of Chlorine.
1.	2·2220	4·1682	46·407
2.	1·9458	3·6512	46·420
	Mean value		46·413

Bringing now the results together,—estimating the amount of oxygen by difference, as is usual in chemical analysis, and calculating what would be the composition of a preparation of antimonious chloride in which $\frac{213}{1000}$ of a per cent. of oxygen had replaced an equivalent amount of chlorine, assuming, of course, Sb = 120 and Cl = 35·5,—we obtain the following very striking accordance:—

	Analysis.	Theory. Sb = 120, Cl = 35·5
Chlorine	46·413	46·418
Oxygen	·213	·213
Antimony	53·374	53·369
	100·000	100·000

The general conclusions, then, which we deduce as the results of this investigation, are—

First, that the value of the atomic weight of antimony found by Schneider in 1856—Sb=120 3—must be accurate within a few tenths of a unit, but that the most probable value of this constant, as deduced from our experiments, is Sb=120, when S=32.

Secondly, that the apparent disagreement with this result, presented by the partial analyses of antimonious chloride, is *probably* due to the constant presence of oxichloride in the preparations of this compound.

The investigation from the first has been a study of constant errors; and those who have followed us through the details will certainly allow that the opinions expressed at the beginning of this paper (on page 42) were not hastily conceived, even if they do not fully agree with our conclusions. In the attempts to correct or balance such errors, we have found at once the chief difficulties and interest of our work, and the secondary results thus reached seem to us the most important fruit of the whole investigation. Seeing, then, the sources of constant error we have discovered, and knowing that there are others whose influence we have been able to trace, although we have not been able to define them as clearly as we could desire, it would be presumptuous in us to express too great confidence either in the correctness of our theories or even in the conclusiveness of our experimental results. Of this, however, we feel assured, that more trustworthy results cannot be expected from a repetition of the same processes until a more complete and accurate knowledge has been acquired of the substances employed. We have therefore proposed to ourselves a more thorough investigation of the haloid compounds of antimony, and the first results of this investigation we shall shortly publish. After the requisite data have been thus collected, we hope to return to the old problem with such definite knowledge of the relations involved as will enable us to obtain at once more sharp and decisive results than are now possible.

During the course of this investigation, we have been successively aided in the experimental work by Dr. F. A. Gooch, Mr. C. Richardson and Mr. W. H. Melville, at the time students in this laboratory; and without their assistance we could not have accomplished the great amount of labor it involved.

Harvard College Laboratory, June 12th, 1877.

ART. XVI.—*Descriptions of two new species of Primordial Fossils;* by S. W. FORD.

Protocyathus rarus, gen. et sp. nov.

THE fossil form for which the above generic and specific names are proposed belongs to the *Archæocyathus* group, and finds its nearest analogue in *Archæocyathellus* of the writer from the same locality and geological horizon.* The only specimen at present known to me is but 0·22 of an inch in length and has a width of only 0·16 of an inch at the larger extremity. The general form is that of a minute cone with the apex broken off. The width at the smaller extremity is 0·12 of an inch, and of this fully one-third is occupied by the cup. The cup itself is filled with light colored limestone, rendering it easily distinguishable from the interseptal areas, which are filled with a darker colored material. These latter appear to have been twenty-eight in number. The radiating septa may be observed in two or three places, and are seen to be thin and delicate. The outer wall has been almost wholly removed and the portions of it that remain are much weathered. The material presented for study consists, therefore, of the solid moulds of the interseptal spaces, the cup filled with limestone, a small number of the septa, a transverse section of the inner wall and the impression of a considerable portion of the outer wall. The latter shows that the external surface when perfect was longitudinally furrowed as in *Archæocyathellus.* In that genus, however, so far as known, there are two rows of pores along each of the furrows, one on either side of the septa; whereas, in the present genus there appears to have been but one, and that placed directly on the line of the septa. The evidence of this consists of rudely circular holes placed at regular intervals along the middle of each furrow in the cast. These appear to me to argue the existence of funnel-like projections inward of the outer wall at the place of the openings. That they mark the position of orifices leading into the interior appears to me in the highest degree probable. Their position is, however, so remarkable, that I was for a long time unable to understand the meaning of them.

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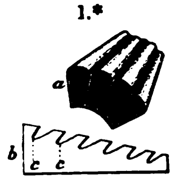
* This Journal, March, 1873.

On one side of the specimen there are a small number of the interseptal moulds that project beyond the others and one of these shows one of its lateral faces for a considerable distance lengthwise, and also nearly down to the outer surface of the inner wall. An examination of this face shows that the cavities observed along the furrows extend but a short distance inward, and that the septa arched around the funnel-like projections which they represent from below, striking the outer wall only at the intervening spaces (the spaces between the dots in the figure). It is further shown that these cavities are directed slightly upward or toward the aperture of the cup. These characters serve to distinguish the form at once from *Archæocyathellus* in which the septa meet the outer wall uninterruptedly.

1.*



If I am right in viewing the cavities along the furrows in the cast as indicating the presence of external orifices at these points, then it follows that these orifices were doubtless functionally equivalent to the double row of orifices along the furrows of the outer wall of *Archæocyathellus.* In proof of this it may be remarked that the size of the cavities indicates that the orifices were proportionally considerably larger than those or the only known species of *Archæocyathellus* (*A. Rensselaericus*), while their position is such as to present no obstacle in the way of regarding them as having communicated simultaneously with two of the interseptal spaces.

The fossils of this group have, in their septate structure much the appearance of corals; but the peculiar poriferous structure known to characterize a good typical species, the uniform presence of a large walled central cavity, and the existence in one species (*Archæocyathus Minganensis*) of branched spiculæ would seem to ally them more nearly to the sponges. By the late lamented Mr. Billings, to whom we owe our first knowledge of these singular forms, and who has discussed their affinities at considerable length, they were classed provisionally with the sponges; but in conclusion he remarks that "The resemblance between the whole structure and that of the paleozoic corals seems also to show that in the Lower Silurian seas forms existed combining the characters of the Protozoa and the Cœlenterata."* The existence of such forms in our older deposits is a matter of much interest, and it is to be hoped that continued researches will add still further to our knowledge of them.

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* **Fig. 1a.—A few of the interseptal moulds of *Protocyathus rarus* enlarged to show the position of the supposed external orifices; *b*, enlarged outline of a lateral face of one of the moulds designed to show the direction of the cavities *c*, *c*.**

This species occurs in conglomerate-limestone of the Lower Potsdam group at Troy, N. Y.

Solenopleura nana, sp. nov.—Of this species I have a number of specimens of the head, but they are all more or less imperfect. The largest and best preserved specimen consists of a nearly perfect glabella and the greater portion of the fixed cheeks, and is but two lines in length. The glabella is nearly four-fifths the total length of the head and is especially characterized by its great relief. It is obtusely conical, slightly widest behind, and is well defined all around by the dorsal furrows. In a specimen two lines in length its highest point is nearly one and one-half lines above the base of the fixed cheeks. It is marked on either side by two or three faint furrows. The fixed cheeks are notably convex, but their relief does not exceed one-third of that of the glabella. The eyes are situated slightly in advance of the mid-length and are connected with the front of the glabella by an obscure ocular fillet. The distance from the eye to the glabella is nearly equal to the width of the glabella at the mid length. The front margin is narrow and is bounded by a feebly convex rim, inside of which there is a narrow furrow which gradually deepens on either side of the median line in passing outward. Between this furrow and the glabella there is a somewhat angular ridge which widens in passing outward to the sutures.

The course of the facial suture is nearly the same with that of *Solenopleura brachymetopa* of Angelin (Palæontologia Scandinavica, Pl. xix, fig. 1), but is directed slightly more inward in front of the eye. The neck-furrow is continuous all across. The exact form of the neck-segment cannot be made out, owing to the damaged condition of all of the specimens at this point. It is seen, however, to be less elevated than in the majority of the species, not rising above the surface of the fixed cheeks. The entire surface is covered with a fine regular granulation.

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* Pal. Foss., vol. i, p. 357.

This species is the second one of the genus, so far as I am aware, that has been described from American rocks, the first one having been obtained from strata of the Acadian epoch in Newfoundland and described by Mr. Billings. It occurs in both even-bedded and conglomerate limestone of the Lower Potsdam group at Troy, N. Y., associated with *Olenellus*, *Conocoryphe* and *Microdiscus*. It is principally interesting on account of its affording another generic link between the already closely related faunæ of the Acadian and Lower Potsdam.

New York, Oct. 13th, 1877.

ART. XVII.—*Note on Lingulella cœlata*; by S. W. FORD.

THE above mentioned species, occurring in the Troy Primordial, has hitherto been set down by me as an *Obolella*, but the evidence now in hand shows that it should be referred to the genus *Lingulella* of Salter. The following are the principal characters:

The ventral valve is somewhat elongate-ovate, with the beak pointed, slightly elevated and conspicuously channeled for the passage of the pedicle. The convexity is moderate and nearly uniform. On the inside there are two prominent, elongate, curved scars, one on either side of the median line, with their concavities directed outward. These recall by their form and position the large lateral scars of the ventral valve of certain species of *Obolella* (e. g., *O. chromatica*.) The other impressions of this valve have not been made out.

The dorsal valve is more rotund than the ventral and has the beak much depressed. The convexity increases with increasing age, and in adult specimens is such as to sometimes give the valve a semi-globose appearance. A shallow depression extends in all the specimens from the beak to the front margin, but in fully grown forms it is often inconspicuous. On the inside there are four prominent ridges. Of these the more central two commence close to the median line a short distance in front of the beak and extend into the forward third of the shell slightly diverging throughout, while the lateral pair take their rise close to the beak and reach to points a little in advance of the mid-length. There is also a short slender ridge directly beneath the beak on the median line. The central portion of the valve in the upper half is slightly excavated. The description of the interior of this valve has been mainly drawn up from an excellent natural internal mould.

The surface of both valves is ornamented with moderately conspicuous radiating and concentric lines, the latter irregularly grouped, and covering the whole a fine papillose network, the points of which are arranged in concentric series, those of one series alternating with those of the next, and so on, as first pointed out by Professor Hall in his description of the dorsal valve (Pal. N. Y., vol. i, p. 290, pl. 79, fig. 9). The effect of this style of ornamentation is very beautiful; and when, as is usually the case, the shells have a dark, polished aspect, with a setting of light-colored limestone, few handsomer fossil objects can be named. The shell is thick and of a finely lamellar structure. The usual length of the ventral valve is about three and one-half lines.

This is one of the best marked fossils of the Troy Primordial and may be easily identified by means of very small fragments.

The species known as *Obolella crassa* of the Troy beds may also be briefly noticed in this connection. It includes the species already widely known under the name of *O. desquamata* from the same locality, this latter, as may be shown, having been founded upon the dorsal valve of the former. The ventral valve is always more acutely pointed at the beak than the dorsal, but beyond this feature there is nothing, so far as I have been able to discover, by which they may be distinguished from each other externally. The surface of each, when perfect, is both radiately and concentrically striated. As a rule, however, the imbricating edges of the successive layers of growth are the only markings visible.

Of the interior of the ventral valve an excellent figure was given by Mr. Billings on page 355 of this Journal for May, 1872; but the interior markings of the dorsal valve have nowhere, to my knowledge, yet been accurately shown. The scars are nearly the same with those of the dorsal valve of *O. chromatica*,* but the smaller pair close to the beak are here, in the majority of cases, distinctly connected with the larger pair directly beneath them; while the central pair, instead of running parallel with each other throughout, diverge at the mid-length of the valve, and extend onward in slender falcate forms into the anterior fourth of the shell. Their parallel portions are, however, the only parts usually seen, and it was only after collecting the species for a number of years that I obtained evidence that what had come to be looked upon as wholes were, in reality, only parts of much more extensive impressions.

The species of Brachiopoda at present known to me from the Troy Primordial are the following: *Obolella crassa*, *O. gem-*

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* "On the structure of *Obolella chromatica*," by E. Billings, F.G.S. This Journal, March, 1876.

ma[*] (Billings sp., or a species which I am unable to distinguish from this form by any good characters), *O. nitida*, *Lingulella cœlata*, and a small species of *Orthis* yet undescribed. This latter species is about one-third smaller than *Orthis Billingsi* of the Acadian group[†] which it otherwise much resembles, except that the ribs do not dichotomize as in that species. None of the specimens yet obtained are sufficiently perfect to admit of a full description.

New York, Oct. 31st, 1877.

ART. XVIII.—*Note on the Development of Olenellus asaphoides*; by S. W. FORD.

SINCE the publication of my paper giving an account of the metamorphoses of this remarkable trilobite,[‡] I have obtained at Troy a number of specimens further illustrating and confirming the fact of the metamorphoses. Among the more important of these is a beautifully preserved cephalic shield showing the manner in which the appendages that I have called the inter-ocular spines were finally lost. As this specimen supplies one of the most important links in the demonstration and fully confirms what was inferred from the structure of previously known specimens representing other phases of the development, the more interesting features which it presents may be briefly noticed at this time.

The specimen in question is almost exactly intermediate between the forms represented by figures 3 and 4 of my former paper. Excepting the neck-furrow and the second pair of furrows in advance, none of the glabellar furrows reach the median line; while the inter-ocular spines, still further reduced in size, are seen to be entirely cut off from the swollen spaces between the eye-lobes and glabella, by the furrows immediately within the eye-lobes extending completely across them, and uniting with the marginal furrows. There can be scarcely a doubt but that the next moult would result in a head destitute of these appendages, as in fact, we find the forms next in order of increasing size to be. The dwarfed proportions of the appendages lead also to the conclusion that they are examples of atrophied organs, as has likewise been suggested to me by M. Barrande. I know of no instance of this—the suppression of spinous appendages beyond the contour—in any other species of trilobite.

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[*] This Journal, May, 1872, p. 355.

[†] Acadian Geol., Dawson, 1868, p. 644. Also Dana, Man. of Geol., 1874, p. 174, fig. 250.

[‡] This Journal, April, 1877.

The surface of the cheeks in the specimen under notice is beautifully ornamented with fine, waved, radiating lines as in the adult.

I may also add, that several of the different stages of growth observed are shown to be represented by two distinct forms, respectively a long and a broad form. The same thing has been stated by Barrande for a large number of Bohemian species. Some of the earlier forms were very diminutive, the smallest specimen now in my possession being one-third smaller than the smallest example yet illustrated. I have also observed one specimen the width of which did not exceed $\frac{1}{36}$th of an inch.*

New York, December 10th, 1877.

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* Neither the small trilobite recently described by G. Linnarsson under the name of *Paradoxides aculeatus* (Transactions of the Geol. Soc. of Stockholm for 1877, p. 359) nor the species previously described as *Paradoxides Kjerulfi* by J. G. O. Linnarsson furnish, in my opinion, any proof of the metamorphoses of Trilobites, or suggest, in their structure, a generic identity with *Olenellus asaphoides*. Moreover, I greatly question whether the two Swedish species above mentioned are truly congeneric, and am strongly disposed to believe that the former will yet turn out to be a genuine *Hydrocephalus*. Mr. Linnarsson notes the close agreement with *Hydrocephalus*, but makes the separation mainly on the strength of his suspicion that Barrande is in error in his diagnosis of the genus. I believe the direction of the facial suture in *P. aculeatus* to be utterly fatal to Mr. Linnarsson's views. There is nothing, to my mind, in the structure of the specimens figured. to lead to a comparison of the posterior spines of *P aculeatus* wtth the smaller pair of *P. Kjerulfi*. In regard to Mr. Linnarsson's somewhat extended, but, as it appears to me, too assured criticism of my work in the publication referred to, I may say that I see no reason to change any of the statements contained in my former paper. Whatever Linnarsson's *Paradoxides aculeatus* may hereafter prove to be, I can confidently assert that *Olenellus asaphoides* is not a true *Paradoxides*. When the development of some species of *Paradoxides* shall have been made out (if, indeed, any of the species ever sustained one) we shall probably be able to satisfy ourselves still more fully upon this point. The smaller spines of the posterior margin of *P. Kjerulfi* suggest a comparison with the inter-ocular spines of *Olenellus asaphoides*, and G. Linnarsson was the first to perceive this; but at present I think it extremely doubtful whether they can be properly regarded as homologous. This will become the more apparent if we compare the figures of *P. Kjerulfi* with my figures 4 and 5. Be this as it may, the relations of the two genera are manifestly very close, and the Swedish beds fortunately promise to contribute largely toward working them out to completeness. Mr. Linnarsson considers *O. asaphoides* to be a true *Paradoxides* and his *P. aculeatus*, one example of which he figures, an embryonic form of the same genus; but finding that the forms of my young specimens do not square with his figure he thinks my account most probably deeply in fault. Were his conclusions and conjectures supported by a better array of facts drawn from his own special field of observation they would possess greater fitness and value.

ART. XIX.—*On Schweitzer's "New Acid Ammonium Sulphates;"* by S. W. JOHNSON and R. H. CHITTENDEN. *Contributions from the Sheffield Laboratory of Yale College.* No. LI.

DR. Paul Schweitzer, in a paper "On some New Acid Ammonium Sulphates, read before the American Chemical Society, July 6, 1876,[*] has given the results of some partial analyses of residues remaining after subjecting ammonium sulphate to several degrees of ignition, and has inferred: 1. That exposure to a heat a little higher than that of the boiling point of mercury converts ammonium sulphate into ammonium bisulphate with loss of one-half of its ammonia. 2. That a temperature somewhat below incipient redness occasions further loss of ammonia and sulphuric acid and leaves a salt of the formula $(NH_4)_2H_4(SO_4)_3$. 3. That probably an intermediate salt is formed having the formula $(NH_4)_4H_2(SO_4)_3$.

These conclusions are based on the fact that the residues of ignition at the temperatures named yield such percentages of SO_3 as the above formulæ require. We have repeated most of Dr. Schweitzer's experiments, and so far as we have gone, have fully verified his observations. The formulæ which he deduces from his estimations of SO_3 are, however, inconsistent with the usually received atom-fixing powers of the elements involved, and we have made further investigation of the substances to which he has called attention, in order to ascertain whether they are really exceptions to the laws of valence, and therefore possibly serviceable means of enlarging our generalizations, or have a composition different from that which Dr. Schweitzer has inferred.

Ammonium sulphate[†] heated for several hours somewhat higher than the boiling point of mercury at first fused without effervescence to a thick pasty mass, as Dr. Schweitzer has stated, but afterwards became fluid. When the fumes no longer had an alkaline reaction, and "ammonia" ceased to be given off, the residue yielded nearly the same percentage of SO_3 obtained by Dr. Schweitzer. He found 69·17 per cent and after fifteen minutes further ignition 69·49 per cent. We found 69·60 per cent, and after further heating at the same temperature 70·03 per cent of SO_3. The bisulphate requires 69·56 per cent. We notice, however, that the per cent of SO_3, which in this case admits of very exact estimation, increases as the ignition is prolonged. A second sample gave in the

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[*] American Chemist, Aug., 1876, p. 42.

[†] Made from sulphuric acid and commercial ammonium carbonate, and analyzed with following results:

	Found.	Calculated.
SO_3	60·70	60·60
$(NH_4)_2O$	39·28	39·39

first analysis 68·95 per cent and after a second ignition 69·41 per cent SO_3. To make the analyses more complete, ammonia was estimated in both samples by distillation with sodium hydroxide, and in the second sample hydrogen was determined by combustion with lead chromate and metallic copper and found to be 4·67 per cent.

On the basis of the ammonia estimation we have the following statement.

	Found. 1.	Found. 2.	Calculated for bisulphate.
SO_3	70·08	69·41	69·56
NH_3	17·00	17·08	14·78
Difference	12·92	13·51	H_2O 15·66
	100·00	100·00	100·00

The ultimate composition, reckoning oxygen by difference, is—

	Found. 1.	Found. 2.	Calculated for bisulphate.
S	28·01	27·76	27·82
O		53·49	55·65
N	14·01	14·08	12·17
H		4·67	4·34
		100·00	99·98

On dissolving in water the solution has an acid reaction, addition of strong alcohol to the saturated solution throws down a crystalline precipitate which is normal ammonium sulphate and yielded in the results of two successive determinations 25·48 per cent and 25·65 per cent of NH_3. Theory requires 25·76 per cent. The alcoholic mother liquor from these determinations gave on evaporation a small crystalline residue, and a few drops of a strongly acid liquid. The absence of any amide or amic acid was shown by the prompt and complete precipitation of all the sulphur by barium salts in cold-prepared solutions. These results would indicate that the supposed bisulphate is a mixture of normal sulphate with bisulphate or pyrosulphate.

The analyzed substance compared with the salts just named in respect to atomic ratios gives the following results, eight atoms of sulphur being assumed in each case for convenience.

Normal sulphate.	Bisulphate.	Pyrosulphate.	Scweitzer's 1st substance.
S= 8	8	8	8
N=16	8	12	9·3
H=64	40	26	43
O=32	32	24	30·8

Inspection of the above figures makes evident that the analyzed substance must contain besides normal sulphate, a certain proportion of bisulphate in order to bring down the nitrogen below twelve and also some pyrosulphate to reduce the oxygen below thirty-two.

Calculation shows, in fact, that the substance is a mixture of nearly one molecule of pyrosulphate $(NH_4)_2S_2O_7$, one molecule of sulphate, $(NH_4)_2SO_4$, and three molecules of bisulphate $3(NH_4HSO_4)$. Such a mixture would have the following empirical expression: $S_6O_{23}N_7H_{31}$ and its centesimal composition compares closely with our analyses.

	Calculated.	Found. 1.	2.
6S	27·86	28·01	27·76
23O	53·41		53·49
7N	14·22	14·01	14·08
31H	4·50		4·67
	99·99		100·00

Our examination of the so-called "biammonium tetrahydrogen sulphate" obtained by subjecting ammonium sulphate to near incipient redness demonstrates that it also is a mixture. The facts given by Professor Schweitzer agree substantially with those observed by us.

He found in the residue after two successive heatings 72·52 and 72·95 per cent SO_3. We found 72·54 and 72·75, and in another sample 71·91 and 72·04 per cent. Our complete analyses do not agree with Professor Schweitzer's formulæ, but indicate that the substance is very nearly a mixture of two molecules of ammonium bisulphate $(NH_4)HSO_4$ with one molecule of pyrosulphate $(NH_4)_2S_2O_7$. Such a mixture is represented empirically by $S_4O_{15}N_4H_{18}$. The percentages required by it and those found in our analyses are subjoined.

	Calculated.	Found. 1.	2.	3.
4S	28·96	29·05	29·01	28·81
15O	54·29	53·85	53·78	
4N	12·67	12·94	12·94	12·96
18H	4·07	4·16	4·27	

This mixture dissolved readily in a small proportion of water, giving a solution acid to test papers and to the taste. The concentrated solution stood for two weeks without crystallizing. On adding a little alcohol (93 per cent) and agitating, oily-appearing drops separated, which shortly united to a heavy layer at the bottom of the vessel, and very soon large prisms appeared in it. More absolute alcohol was added, and after twenty-four hours standing a fine crop of crystals was obtained. These crystals A were rinsed with alcohol and dried at 100° C. To the filtrate absolute alcohol was added as long as a precipitate separated. On standing an abundant deposit of acicular crystals formed. These B were separated and dried at 100 C. These crystals proved to be normal ammonium sulphate.

They yielded by analysis—

	A	B	Calculated.
$(NH_4)_2O$	39·33	39·46	39·39

The alcoholic mother-liquor was evaporated on the water-bath and left a small fluid residue, which on cooling deposited a few crystals, apparently of normal sulphate. The few drops of liquid remaining were intensely acid and had all the characters of sulphuric acid discolored by organic matters. Treatment with aqueous alcohol thus resolves both the bisulphate (two molecules) and the pyrosulphate into normal sulphate and sulphuric acid, or in part probably into sulphethylic acid.

In the first stage of the decomposition of ammonium sulphate at a temperature "a little higher than the boiling point of mercury" the vapors are alkaline. The chemical change would appear to involve six molecules of the sulphate, which lose five molecules of ammonia gas and one molecule of water vapor, leaving as solid residue a mixture of one molecule of unchanged sulphate, one of pyrosulphate and three of bisulphate.

$$6[(NH_4)_2SO_4]-[5NH_3+H_2O]=3[NH_4HSO_4]+(NH_4)_2S_2O_7+(NH_4)_2SO_4$$

In these changes two molecules of sulphate yield one molecule of pyrosulphate with loss of one water- and two ammonia-molecules,* while three molecules of sulphate yield, each, a molecule of bisulphate, with loss of a molecule of ammonia,† and the sixth molecule of sulphate comes out unaltered.

In the second stage of heating (near incipient redness) the fumes are at first alkaline, but shortly become acid, and continued so as long as that temperature is maintained.

The changes are empirically expressed as follows:

$$S_6O_{23}N_7H_{31}-[2SO_3+2H_2O+3NH_3]=S_4O_{15}N_4H_{18},$$

or rationally—

$$[3(NH_4HSO_4)+(NH_4)_2S_2O_7+(NH_4)_2SO_4]-[2SO_3+2H_2O+3NH_3]=2(NH_4HSO_4)+(NH_4)_2S_2O_7.$$

The rise of temperature from 350° to 520° C. appears not to alter the pyrosulphate and bisulphate, but the chemical change seems to result from a molecule of sulphate reacting on a molecule of bisulphate, whereby both are decomposed, thus—

$$NH_4HSO_4+(NH_4)_2SO_4=2SO_3+2H_2O+3NH_3.$$

It would be interesting to study the reactions at other temperatures.

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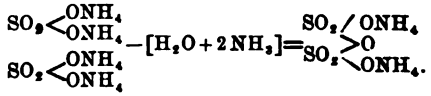
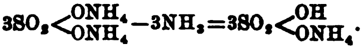
* 

$$\begin{matrix} SO_2<\begin{smallmatrix}ONH_4\\ONH_4\end{smallmatrix} \\ SO_2<\begin{smallmatrix}ONH_4\\ONH_4\end{smallmatrix}\end{matrix}-[H_2O+2NH_3]=\begin{matrix}SO_2/ONH_4\\ \quad >O\\ SO_2\backslash ONH_4.\end{matrix}$$

† 

$$3SO_2<\begin{smallmatrix}ONH_4\\ONH_4\end{smallmatrix}-3NH_3=3SO_2<\begin{smallmatrix}OH\\ONH_4\end{smallmatrix}.$$

ART. XX.—*The Poplars of North America;* by SERENO WATSON.

THE following incomplete synopsis of the species of *Populus* is based upon the material in several of our principal herbaria, and is published for the purpose of drawing the attention of botanists during the coming season to this still very imperfectly known genus. Flowers and fruit even of the common species are too rare in collections, and are much needed for their satisfactory definition.

§ 1. Styles two, with two or three narrow or filiform lobes: capsules small, thin, oblong-conical, two-valved: seeds very small: leaves ovate.

* Petioles flattened: bracts silky: stamens six to twenty.

1. *P. tremuloides* Michx. 2. *P. grandidentata* Michx.

** Petioles terete: bracts not silky: stamens twelve to sixty.

3. *P. heterophylla* L.

§ 2. Styles two to four, with dilated lobes: capsules large, often thick, subglobose to ovate-oblong, two to four-valved: bracts mostly glabrous.

* Leaves cordate or ovate to lanceolate, crenate; petioles terete: stamens twelve to thirty: seed a line long.

† Capsule glabrous, two-valved.

4. *P. balsamifera* L. Leaves whiter beneath, ovate-lanceolate, acuminate, glabrous; petioles one-half to two inches long, at first puberulent: rhachis of aments (pistillate two and one-half to five inches long) pubescent: disk one or two lines broad: stamens twenty to thirty: capsules ovate (three lines long), on very short pedicels.—Var. *candicans* Gray. Leaves broader and cordate: the petioles often somewhat hairy.—Var. (?) *Californica*. Leaves ovate to oblong-lanceolate, acute, usually rounded at base, somewhat pubescent beneath, mostly on short petioles (half inch long or less).—The typical form northward to the Arctic Ocean; var. *candicans* west to Colorado and Idaho; the last variety on the Pacific side from Los Angeles to Oregon, but known only from the foliage.

5. *P. angustifolia* James. Leaves not whiter beneath, rhombic-ovate to narrowly lanceolate, mostly cuneate at base, often small; petioles one-half inch long or less (rarely one inch): rhachis glabrous (pistillate two inches long, rarely two or four): disk a line broad or less: stamens twelve: capsules ovate, smaller, on very short pedicels.—Colorado and New Mexico to Central Arizona and Nevada, and Washington Territory. Two forms are spoken of, the "Yellow Cottonwood," making fair lumber, and the "Black Cottonwood," common

and extensively planted in Utah, but the wood considered worthless.

† † Capsule tomentose, three-valved.

6. *P. trichocarpa* Torr. and Gray. Leaves broadly ovate, acuminate, cordate, often whiter beneath with age, puberulent when young; petioles one or two inches long: rhachis pubescent (pistillate two to six inches long): disk very broad: stamens twenty to thirty: capsule subglobose, nearly glabrous.—Var. *cupulata.* Disk somewhat herbaceous, very large and campanulate, twice longer than the ovary, pubescent: bracts villous: pedicels a line or two long.—S. California to W. Nevada and British Columbia.

* * Leaves deltoid, sinuate-crenate; petioles flattened: stamens sixty or more: seed one and one-half or two lines long: capsule three or four-valved: rhachis and disk glabrous.

7. *P. monilifera* Ait. Leaves with numerous serratures and narrow very acute acumination, broadly truncate-deltoid, sometimes ovate, rarely cordate; petioles two to four inches long: ament usually long (two to seven inches): disk rarely two lines broad: capsules rather thin, oblong-ovate, four or five lines long, on slender pedicels one to five lines long.—New England to Florida, Louisiana, and the base of the Rocky Mountains in Colorado and Wyoming. Most flowering and fruiting specimens seen from east of the Mississippi have four, rarely three, distinct styles and a four-valved capsule; a single specimen from the Agricultural Grounds, Washington (cultivated as *P. angulata*), has the styles united and bearing a peltate stigma, and the capsule three-valved. The more western specimens have all three distinct styles and a three-valved capsule. There are no apparent differences otherwise, and it remains to be seen whether these forms can be specifically separated.

8. *P. Fremonti* Watson. Leaves with few serratures (four to twelve on each side), broadly deltoid with a broad acute apex and usually somewhat reniform or cordate; petioles one to two and one-half inches long, often pubescent (as the branchlets) with short spreading hairs: staminate disk three or four lines broad, and pedicels eight to ten lines long: pistillate aments three or four inches long: disk three lines broad: capsules ovate, thick-coriaceous, three-valved, on stout pedicels two lines long or less.—Var. (?) *Wislizeni.* Leaves sharply acuminate, truncate or slightly cuneate at base: staminate disk less dilated, and the pedicels shorter: pistillate aments very slender (two to six inches long), the disk two or three lines broad, and the somewhat angled capsules three- or usually four-valved, on slender pedicels two to eight lines long.—The typical form from N. California to S. Utah; the variety from S. California to the Rio Grande.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *Liquefaction of Oxygen;*[*] by M. RAOUL PICTET.—The object which I have had in view for more than three years is to demonstrate experimentally that molecular *cohesion* is a general property of bodies, to which there is no exception.

If the permanent gases are not capable of liquefying, we must conclude that their constituent particles do not attract each other, and thus do not conform to this law.

Thus, to cause experimentally the molecules of a gas to approach each other as much as possible, certain indispensable conditions are necessary, which may be expressed thus:—

(1.) To have the gas absolutely pure, with no trace of foreign gas.
(2.) To be able to obtain extremely energetic pressures.
(3.) To obtain intense cold, and to subtract heat at these low temperatures.
(4.) To utilise a large surface for condensation at these low temperatures.
(5.) To be able to utilise the rapid expansion of the gas from extreme condensation to the atmosphere pressure—an expansion which, added to the preceding means, will *compel* liquefaction.

Having fulfilled these five conditions, we may formulate the following alternative:—

When a gas is compressed to 500 or 600 atmospheres, and kept at a temperature of $-100°$ or $-140°$, and it is allowed to expand to the atmospheric pressure, one of two things takes place:—

Either the gas, obeying the force of cohesion, liquefies, and yields its heat of condensation to the portion of gas which expands and loses itself in the gaseous form; or, on the hypothesis that cohesion is not a general law, the gas must pass to the absolute zero and become inert,—that is to say, an impalpable powder.

The work done by expansion will not be possible, and the loss of heat will be absolute.

Struck with the truth of this alternative, which is rendered certain by thermo-dynamic equations based on accurate data, I have sought to produce a mechanical arrangement which should entirely satisfy these different conditions, and I have chosen the complicated apparatus of which the following is a brief description:—

I take two pumps, P_3 and P_4, for exhaustion and compression, such as are used industrially in my ice-making apparatus. I couple these pumps in such a way that the exhaustion of one corresponds to the compression of the other. The exhaustion of the first communicates with a tube (R) of 1·1 metres long and 12·5 centimeters in diameter, and filled with liquid sulphurous acid. Under the influence of a good vacuum the temperature of this liquid rapidly sinks to −65°, and even to —73°, the extreme limit attained.

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[*] The *liquefaction of oxygen* is so important a scientific achievement that we have much pleasures in laying before our readers the following detailed account of the means employed and diagrams of the apparatus used, which have been communicated to us by M. Pictet himself.—ED. CHEMICAL NEWS.

Through this tube of sulphurous acid passes a second smaller tube (S), of six centimeters diameter, and the same length as the envelope. These two tubes are closed by a common base.

In the central tube is retained compressed carbonic acid produced by the reaction of hydrochloric acid on Carrara marble. This gas, being dried, is stored in an oil gasometer (G) of one cubic meter capacity.

At a pressure of from four to six atmospheres the carbonic acid easily liquefies under these circumstances. The resulting liquid is led into a long copper tube (B), four meters in length and four centimeters in diameter.

Two pumps, P_1 and P_2, coupled together like the first, exhaust carbonic acid either from the gasometer (G) or from the long tube (B) full of liquid carbonic acid.

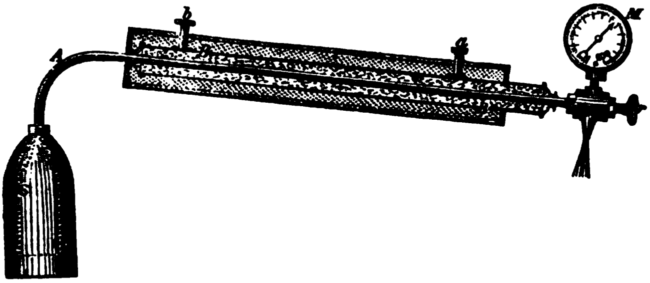
The ingress to these pumps is governed by a three-way tap, H. A screw valve cuts off at will the ingress of liquid carbonic acid in the long tube; it is situated between the condenser of carbonic acid and this long tube. When this screw valve is closed, and the two pumps draw the vapor from the liquid carbonic acid contained in the tube four meters long, the greatest possible lowering of temperature is produced; the carbonic acid solidifies and descends to about −140°. The subtraction of heat is maintained by the working of the pumps, the cylinders of which take out three liters per stroke, and the speed is 100 revolutions a minute.

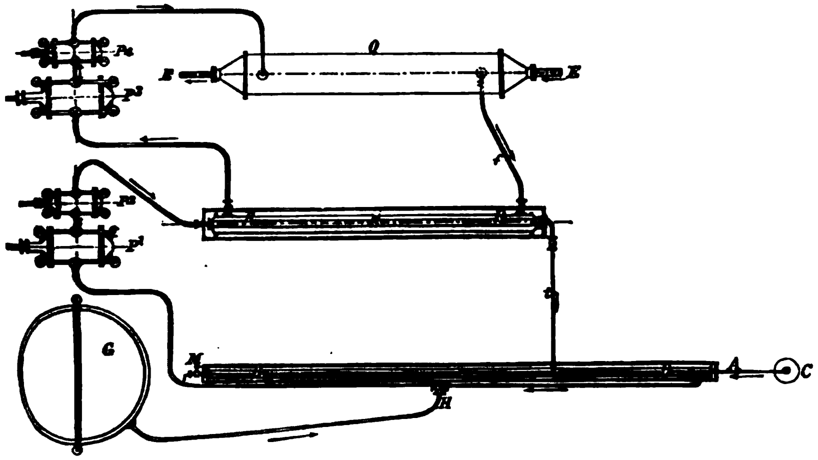
Both the sulphurous acid tube and the carbonic acid tube are covered with a casing of wood and non-conducting stuff to intercept radiation.

In the interior of the carbonic acid tube, B, passes a fourth tube, A, intended for the compression of oxygen; it is five meters long and fourteen millimeters in external diameter. Its internal diameter is four millimeters. This long tube is consequently immersed in solid carbonic acid, and its whole surface is brought to the lowest obtainable temperature. These two long tubes are connected by the ends of the carbonic acid tube, consequently the small tube extends about one meter beyond the other. I have curved this portion downward and given the two long tubes a slightly inclined position, but still very near the horizontal, as I have shown in the accompanying drawing.

The small central tube is curved at A, and screws into the neck of a large howitzer shell, C, the sides of which are thirty-five millimeters thick; the height is twenty-eight centimeters, and the diameter seventeen centimeters.

This shell contains 700 grams of chlorate of potash and 256 grams of chloride of potassium mixed together, fused, then broken up, and introduced into the shell perfectly dry. When the double circulation of the sulphurous and carbonic acids has lowered the





DESCRIPTIONS OF THE DRAWINGS.

A. A tube, 5 meters long, 14 millimeters external diameter, and 4 millimeters internal diameter, in which the oxygen condenses. It is furnished with a screw-tap, *r*, from which the liquid oxygen jets out. A pressure-gauge, M, measures the pressure up to 800 atmospheres.

B. A tube, 4 meters long, in which is solid carbonic acid. The stock of carbonic acid is contained in a gasometer, G, of 1 cubic meter capacity. A three-way tap, H, puts it when desired into communication with the apparatus.

C. A howitzer shell, containing 700 grams of chlorate of potash mixed with chloride of potassium. It is heated with gas.

P_1, P_2. Double-action exhaustion and force pumps, drawing carbonic acid from the tube B or the gasometer G, according to the position of the tap H.

S. A tube, 60 millimeters diameter and 1·1 meter long, in which is condensed the liquid carbonic acid compressed by the pumps. This liquefied gas returns by the small tube *t* to the tube B.

R. A tube, 125 millimeters in diameter and 1·1 meters long, containing liquid sulphurous acid.

P_3, P_4. Double-action exhaustion and force pumps, exhausting sulphurous acid gas from the tube R.

Q. A tubular condenser of sulphurous acid compressed by the pumps. This body, when liquefied, returns by the small tube *f* to the tube R. The cold water for condensing the sulphurous acid passes through the apertures E E.

a. Entry for liquid carbonic acid.

b. Exit for the vaporised carbonic acid caused by the suction of the pumps.

temperature to the required degree, I heat the shell over a series of gas-burners. The decomposition of the chlorate of potash takes place at first gradually, then rather suddenly towards the end of the operation. A pressure-gauge, M, at the extremity of a long tube, lets me constantly observe the pressure and the progress of the reaction. This gauge is graduated to 800 atmospheres, and was made for me expressly by Bourdon, of Paris.

When the reaction is terminated the pressure exceeds 500 atmospheres; but it almost immediately sinks a little, and stops at 320 atmospheres. If at this moment I open the screw-tap, *r*, which terminates the tube, a jet of liquid is distinctly seen to spirt out with extreme violence. I close the tap, and in the course of a few moments a second jet—less abundant, however—can be obtained.

Pieces of charcoal, slightly incandescent, put in this jet inflame spontaneously with inconceivable violence. I have not yet succeeded in collecting the liquid, on account of the considerable projectile force with which it escapes, but I am trying to arrange a pipette, previously cooled, which possibly may be able to retain a little of this liquid.

Yesterday I repeated this experiment before the majority of the members of our Physical Society, and we had three successive jets, well characterized. I cannot yet determine the minimum pressure necessary, for it is evident that I have a surplus pressure produced by the excess of gas accumulated in the shell, and which could not condense in the small space represented by the interior tube.

I hope to utilise a similar arrangement in attempting the condensation of hydrogen and nitrogen, and I am especially occupied with the possibility of maintaining low temperatures very easily, thanks to four large industrial pumps which I have at my disposal, worked by a steam-engine.

Geneva, December 25, 1877.

Since receiving the above we have been favored with further particulars of an experiment which was performed for the fourth time on Thursday, December 27th, in the presence of ten scientific men—among others, Professor Hagenbach, of Bâle, who came expressly to assist at this important experiment.

At 10 o'clock in the evening the manometer, which had risen to 560 atmospheres, sank in a few minutes to 505, and remained stationary at this figure for more than half-an-hour, showing by this diminution in the pressure that part of the gas had assumed the liquid form under the influence of the 140 degrees of cold to which it was exposed. The tap closing the orifice of the tube was then opened, and a jet of oxygen spirted out with extraordinary violence.

A ray of electric light being thrown on the escaping jet showed that it was chiefly composed of two parts;—one central, and some centimeters long, the whiteness of which showed that the element was liquid, or even solid; the other exterior, the blue tint of which indicated the presence of oxygen compressed and frozen in the gaseous state.

The success of this remarkable and conclusive experiment called forth the applause of all present.

We understand that Messrs. Pictet & Co., of 22, Rue de Grammont, Paris, are fitting up apparatus with the intention of having these experiments repeated at their Freezing-Machine Works, at Clichy, in Paris.—*Chemical News, Jan.* 4.

2. *The Liquefaction of Oxygen, Nitrogen and Hydrogen.* Experiments of M. Cailletet. (From Nature, of January 3d.)— . . . It appears that as early as December 2, M. Cailletet had succeeded in liquefying oxygen and carbonic oxide at a pressure of 300 atmospheres and at a temperature of −29° C. This result was not communicated to the Academy at once, but was consigned to a sealed packet on account of M. Cailletet being then a candidate for a seat in the Section of Mineralogy. Hence, then, the question of priority (between M. Cailletet and M. Pictet) has been raised, but it is certain that in the future the work will be credited to both, on the ground that the researches of each were absolutely independent, both pursuing the same object, creating methods and instruments of great complexity. . . .

The methods employed by MM. Pictet and Cailletet are quite distinct and are the result of many years' preparatory study, as testified by M. H. St. Claire Deville and M. Regnault. It is difficult to know which to admire most, the scientific perfection of Pictet's method or the wonderful simplicity of Cailletet's. It is quite certain that the one employed by the latter will find frequent use in future experiments. M. Cailletet's apparatus consists essentially of a massive steel cylinder with two openings; through one hydraulic pressure is communicated. A small tube passes through the other, the sides of which are strong enough to withstand a pressure of several hundred atmospheres, and which can be inclosed in a freezing mixture. It opens within the cylinder into a second smaller cylinder serving as a reservoir for the gas to be compressed. The remainder of the space in the large cylinder is occupied by mercury. M. Cailletet's process consists in compressing a gas into the small tube, and then by suddenly placing it in communication with the outer air, producing such a degree of cold by the sudden distention of the confined gas that a large portion of it is condensed, a process perfectly analogous to that used to prepare solid carbonic acid by the rapid evaporation of the liquefied gas.

In M. Cailletet's experiment with oxygen it was brought to a temperature of −29° C. by the employment of sulphurous acid and a pressure of 300 atmospheres; the gas was still a gas. But when allowed to expand suddenly, which, according to Poisson's formula, brings it down to 200° below its starting point, a cloud was at once formed. The same result has since been obtained without the employment of sulphurous acid, by giving the gas time to cool after compression. M. Chailletet has not yet obtained, at all events, so far as we yet know, oxygen in a liquid form, as

M. Pictet has done; on being separated from its enormous pressure it has merely put on the appearance of a cloud. . . .

M. Cailletet first introduced pure nitrogen gas into the apparatus. Under a pressure of 200 atmospheres the tube was opened, and a number of drops of liquid nitrogen were formed. Hydrogen was next experimented with, and this, the lightest and most difficult of all gases, was reduced to the form of a mist at 280 atmospheres. The degree of cold attained by the sudden release of these compressed gases is scarcely conceivable. The physicists present at the experiment estimated it at —300° C.

Although oxygen and nitrogen had both been liquefied, it was deemed of interest to carry out the process with air, and the apparatus was filled with the latter, carefully dried and freed from carbonic acid. The experiment yielded the same result. On opening the tube a stream of *liquid air* issued from it resembling the fine jets forced from our modern perfume bottles.

These more recent results are all the more surprising as, at an earlier stage, hydrogen, at a pressure of 300 atmospheres, has shown no signs of giving way.

3. *On the Liquefaction of Acetylene, Ethyl hydride, Nitrogen dioxide and probably Marsh gas.*—CAILLETET, studying the compressibility of acetylene, observed a marked departure from the law of Mariotte in its behavior, and, pushing the condensation still further, succeeded in liquefying it. The apparatus consisted of a hollow steel cylinder, thick enough to resist a pressure of several hundred atmospheres, having screwed into it at top a bronze collar carrying a thick glass reservoir closed above, and continued downward by a larger tube, open at bottom. This glass tube contains the gas to be liquefied, its lower end dipping below the surface of mercury in the steel cylinder. Hydraulic pressure forces the mercury up into the tube, compressing the gas and finally liquefying it in the narrow portion at top. In compressing acetylene, the temperature being +18°, numerous droplets are seen to form and run down the walls of the tube, under a pressure of eighty-three atmospheres. Reducing the pressure gradually, the liquid returns suddenly to gas filling the tube for an instant with a thick fog. Liquid acetylene is colorless and extremely mobile; it appears to be highly refractive, and is lighter than water in which it is soluble in all proportions. It dissolves paraffin and fats. Cooled to zero in presence of water and linseed oil, it forms a white compound like snow, which decomposes on heating or lowering the pressure. The tension of the acetylene vapor is as follows: at +1°, 48 atmospheres; at 2·5, 50; at 10, 63; at 18°, 83; at 25°, 94; at 31°, 103. Comparing the tensions of acetylene, ethylene, and ethylene hydride (C_2H_6) which contain in equal volumes, equal weights of carbon united to increasing quantities of hydrogen in the ratio 1 : 2 : 3, the author finds the tension of acetelyne at 1°, as above, to be forty-four atmospheres; that of ethylene at 0° according to Faraday, being forty-six atmospheres, and that of ethylene hydride—now liquefied for

the first time by Cailletet—at 4° being forty-six atmospheres, its liquefaction taking place at a pressure a little less than that of acetylene.

In a subsequent paper, CAILLETET announces the liquefaction of nitrogen dioxide, by a pressure of 104 atmospheres at −11°. At 8°, the dioxide is still gaseous under a pressure of 270 atmospheres. Marsh gas, compressed by 180 atmospheres at 7°, shows, when the pressure is suddenly diminished, a thick fog like that given by carbon dioxide under similar circumstances; whence the author hopes to effect its liquefaction.—*C. R.*, lxxxv, 851, 1016, Nov. 1877. G. F. B.

4. *On Flame-temperatures.*—ROSSETTI has made a series of experiments to determine the actual temperature of a flâme by direct measurement. For this purpose he used a thermo-pile consisting of a pair of wires 0 3 millimeter in diameter, and forty centimeters long, one of platinum the other of iron, both enclosed in porcelain tubes, the ends, which projected about two millimeters, being united and then covered with kaolin sintered together. The free ends of the two wires were soldered to large copper wires, which ran to the galvanometer. The graduation of the apparatus was effected by means of thermometers for low temperatures; and for high ones, not exceeding, however, 825°, by means of a calorimeter. Since the curve given by these determinations was almost a straight line, especially the latter portions of it, Rossetti believed that he might continue it beyond the experimental limit in the same direction without marked error. In the flame of an ordinary Bunsen burner, this flame being seventeen centimeters long, he found the highest temperature, 1350° C;, to be in the colorless mantle of the flame, the violet zone having a temperature of 1250°, and the inner blue flame not reaching 1200°. In the dark non-luminous cone in the center, seven centimeters long, the temperature one centimeter above the opening of the burner was only 250°; at two centimeters above, it became 400° and so remained up to a height of four or five centimeters, reaching 650° at the height of six centimeters. Mixtures of air and gas were then burned in a Bunsen burner closed below, and the temperature measured in the hottest part of the flame. For one volume of gas and two volumes of air, this temperature was 1260°; for one volume of gas and two and one-half of air, 1150°; and for one volume of gas and three of air, 1116°. With four volumes of air the mixture would no longer burn in a Bunsen burner; and burned from a bat-wing burner gave a maximum temperature of only 958°. Mixtures of gas and carbon dioxide, burned in a Bunsen burner as above, gave, for one volume gas and one and one-half CO_2, a temperature of 1000°; one volume gas and two of CO_2 gave 860°; and with three of CO_2 780°. With four volumes CO_2, the mixture burned only in contact with a flame.—*Gaz. Chim. Ital.*, vii, 422, Sept. 1877. G. F. B.

5. *On the principle of Maximum Work, as illustrated by the Spontaneous decomposition of Barium perhydrate.*—As a funda-

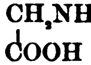
mental deduction from his thermochemical researches, **Berthelot** proved long ago the tendency of chemical systems toward that composition which corresponds to the maximum evolution of heat. He now notes an excellent illustration of this law in the case of barium perhydrate, which decomposes spontaneously, while barium peroxide is permanent. A specimen of BaO_2 prepared in 1874 contained 9·4 oxygen in excess, and in 1877, 9·2 of this oxygen; showing its permanence. The hydrate however, $BaO_2(H_2O)_7$, prepared pure and kept moist, gradually decomposes, gas bubbles of oxygen developing in the mass, generating a pressure in the vessel, and forming a crystalline mass of barium hydrate $BaO, (H_2O)_{10}$. This decomposition is even more rapid under water. A specimen prepared in 1874 and kept moist, had lost in 1877 a fifth of its oxygen; while another portion kept under water had lost nearly all. In explanation of these facts, Berthelot shows: (1) that barium dioxide absorbs heat in decomposing, $BaO_2=BaO+O$ absorbing 12·1 calories, and hence cannot decompose without the aid of foreign energy; (2) that the decomposition of barium dioxide or perhydrate into barium hydrate and free oxygen, on the contrary, evolves heat, $BaO_2+H_2O=Ba(OH)_2+O$ evolving 5·52 calories if the water be considered liquid, or 4·0 if solid; and hence, as Boussingault has observed, aqueous vapor can displace the oxygen, the heat evolved being 15·2 calories, since the two bodies are in the same physical state. In the same way $BaO_2(H_2O)_7+(H_2O)_3=BaO(H_2O)_{10}+O$, evolving 10·6 calories, or 6·4, as the water is liquid or solid. Hence barium perhydrate decomposes spontaneously and at the ordinary temperature. "Thus," says Berthelot, "the secret of spontaneous decomposition of barium perhydrate is not to be found in any symbolic considerations, drawn from a figurative arrangement of atoms; but is explained by very simple and very obvious principles, resulting from the regular action of molecular mechanics."—*Bull. Soc. Ch.*, II, xxviii, 502, Dec. 1877. G. F. B.

6. *On the Hydrocarbon called Idryl.*—**Goldschmiedt** has submitted to extended examination the mixture of hydrocarbons found in the quicksilver chambers of Idria, from which Bödecker obtained the hydrocarbon which he called idryl. The crude material was the alcoholic extract of the chamber deposit, and fused from 75° to 86°. By solution in alcohol, difficultly soluble flocks were observed which were filtered off and marked A. They fused at about 200°. From the filtrate, or from the more fusible portions of the crude material, only very small quantities of A were obtained. On distilling the raw product in a current of CO_2, a white substance was obtained fusing at 100°, which was crystallized from alcohol and by sublimation (B). The residues were dissolved in alcohol and mixed with a boiling saturated alcoholic solution of picric acid. Red crystalline precipitates were thus obtained, most abundantly from the portions of lower melting point. On concentration, additional picrate was obtained but lighter in color as it was more soluble. The portions having

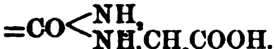
nearly the same fusing point were united and recrystallized till this point was constant; eighty fractions being in this way condensed into three. The first (C) was in dark red flexible needles, fusing at 220°; the second (D) was in large bright red brittle needles, fusing at 185°; the third (E) was in gold-yellow fine delicate needles, which fused at 144°. On examining the fractions obtained A yielded a small quantity of a body insoluble in benzene and probably chrysene; the larger portion was anthracene. The fraction B was phenanthrene, the fraction C pyrene, and the fraction D a new hydrocarbon having the formula $C_{15}H_{10}$, which though not identical with Bödecker's substance—this being probably a mixture of pyrene and phenanthrene—the author proposes to call idryl. Further researches upon its constitution are in progress.—*Ber. Berl. Chem. Ges.*, x, 2022, Dec. 1877. G. F. B.

7. *On the Determination of Nitrogen in Nitroglycerin.*—Lauer and Ador have made a series of experiments to ascertain the best method of determining nitrogen in nitroglycerin. Solution of the nitroglycerin from a dynamite with ether, evaporation of the ether and solution in potassium hydrate in excess, and then determination of the nitrogen by Reichhardt's method, gave too low results. The dynamite was then shaken with water, the deposited nitroglycerin dissolved in alcohol, the alcoholic solution treated with potash and the nitrogen determined as above; this method also gave too low results. Finally 30 grams copper oxide were mixed with the nitroglycerin and burned by Dumas's method. The combustion went on quietly and yielded the theoretical quantity, 18·5 per cent.—*Ber. Berl. Chem. Ges.*, x, 1982, Dec. 1877. G. F. B.

8. *On Aromatic Hydantoins.*—By the direct union of cyanic acid and glycocoll, hydantoic acid is produced:

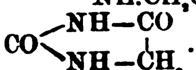


$\begin{matrix}CH_2NH_2\\ |\\ COOH\end{matrix}$ + CONH



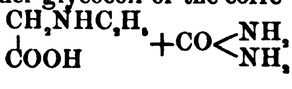
$=CO<\begin{matrix}NH_2\\ NH.CH_2COOH.\end{matrix}$

By loss of water, hydantoin is formed

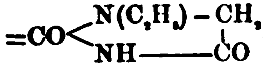


$CO<\begin{matrix}NH-CO\\ \quad\ \ |\\ NH-CH_2\end{matrix}.$

Hydantoin and its homologues methyl-hydantoin, may also be formed by fusing together glycocoll or the corresponding homologues of it with urea:

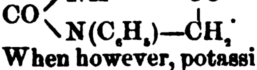


$\begin{matrix}CH_2NHC_2H_5\\ |\\ COOH\end{matrix} + CO<\begin{matrix}NH_2\\ NH_2\end{matrix}$



$=CO<\begin{matrix}N(C_2H_5)-CH_2\\ \qquad\qquad |\\ NH\text{———}CO\end{matrix} + H_2O + NH_3.$

Schwebel has succeeded in forming a phenyl-hydantoin by fusing together phenyl-glycocoll with urea:



$CO<\begin{matrix}NH\text{———}CO\\ \qquad\qquad |\\ N(C_6H_5)-CH_2\end{matrix}.$

Phenyl-hydantoic acid was not obtained. When however, potassium cyanide, ammonium sulphate and phenyl-glycocoll in aqueous solution are allowed to stand for some days at 40° the filtrate after separation of the potassium sulphate by alcohol, yielded abundance of phenyl-hydantoin.—*Ber. Berl. Chem. Ges.*, x, 2045, Dec. 1877. G. F. B.

9. *On the Behavior of Benzoic acid in the Organism of Birds.*—JAFFE has taken up anew the question of the change which benzoic acid undergoes in the organism of birds, first investigated by Meissner and Shepard. He confirms the result of these latter chemists that no hippuric acid is formed, but that an acid is excreted which like hippuric acid is a paired benzoic acid. To this acid he gives the name *ornithuric* acid. He prepares it by extracting with alcohol the fresh excreta of hens fed on benzoic acid, evaporating the alcohol, extracting again with hot absolute alcohol, and evaporating. The strongly acid liquid is mixed with water, and repeatedly agitated with ether, to remove fatty impurities. The residue is treated with dilute sulphuric acid, and again agitated with more ether. The etherial solutions are concentrated and allowed to stand for some days in a cool place. The ornithuric acid separates in crystalline masses, having when pure the empirical formula $C_{19}H_{20}N_2O_4$. Boiled with hydrochloric acid it gives benzoic acid and a new base $C_5H_8(NH_2)_2O_2$, diamidovaleric acid.—*Ber. Berl. Chem. Ges.*, x, 1925, Nov. 1877. G. F. B.

9. *Le Sage's Theory of Gravitation*; by JAMES CROLL, LL.D., F.R.S.—Le Sage's Theory of Gravitation is at present exciting a good deal of attention among physicists. This is perhaps to a considerable extent due to the fact that some of the conditions arbitrarily assumed by Le Sage in his hypothesis, have been proved to follow as necessary consequences from the kinetic theory of gases.

A clear and able account of this theory of gravitation has been given by Mr. Preston in the Philosophical Magazine for September and November last. Mr. Preston has endeavored to answer all the objections which have been urged against the theory; but in one case at least he seems to me to have failed.* It is a necessary condition of Le Sage's theory, in order that gravity may be proportional to mass, that the total volume of the free spaces in a substance in the form of interstices between the molecules must be great compared with the total volume of matter contained in the molecules themselves. This condition of free interstices Mr. Preston considers to be satisfied by assuming the molecules to be small as compared with their mean distances.

Were we at liberty to make any assumptions we chose in reference to the smallness of the molecules of matter and their distance apart, we might be able to satisfy the conditions required as to mass. This, however, we are not at liberty to do, for modern physics has enabled us to determine, at least roughly, the size of the ultimate molecules of matter and also the distances between them. This subject has recently been investigated by Sir William Thomson, who has given full details of his result in a remarkable paper in "Nature" vol. i, p. 551. Sir William says the diameter of the molecule cannot be less than $\frac{1}{500,000,000}$ of a centimeter. The number of molecules in a cubic centimeter of a liquid or a solid may, he says, be from 3×10^{24} to 3×10^{26}. This gives the distance from center to center of two consecutive molecules to be from $\frac{1}{140,000,000}$ to $\frac{1}{480,000,000}$ of a centimeter. Now, if we take the mean of these two values we have $\frac{1}{300,000,000}$ of a centimeter for the distance between the centers. The mean *spaces* between the molecules are therefore less than the diameter of the molecules themselves. Under this condition of things it must be absolutely impossible that a gravific particle even though it were *infinitely small* could penetrate to the extent of a thousandth part of a centimeter, into the interior of a body without having its motion stopped by coming into collision with a molecule. Le Sage's theory appears therefore to be utterly irreconcilable with Sir William's conclusions regarding the size of the material molecule. But even supposing we were to assume, what we are hardly warranted in doing, that the molecules are 10,000 times smaller and their distances 10,000 times greater than Sir William Thomson concludes, still this would not assist the theory. The gravific particles would then, no doubt, penetrate a little further into the interior of a body, but beyond a few feet or perhaps a few inches no particle could go. (Communicated by the Author.)—*Phil. Mag.*, Jan., 1878.

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* In an interesting article on Kinetic Theories of Gravitation by Mr. W. B. Taylor published in the Smithsonian Report for 1876, he lays down six fundamental characteristics of gravitation with which every theory, he says, must agree. Of these six requirements, Le Sage's theory he maintains satisfies but two, namely, (1) that the direction of gravity is radial toward the acting mass, and (2) that its intensity diminishes as the square of the distance. But some of Mr. Taylor's objections have already been met by Mr. Preston in his memoir. Besides one or two of Mr. Taylor's fundamental postulates seem doubtful.

10. *On the Thermal Conductivity and Diathermancy of Air and Hydrogen.*—Dr. HENRY BUFF, Professor of Physics in the University of Giessen has undertaken the revision of the work of Magnus, Tyndall and others upon this subject. The apparatus used was similar to that of Magnus, which is essentially as follows: Upon a vertical cylinder of thin glass, 56 mm. in diameter and 160 mm. in height, there was fused another cylinder of the same diameter but of 100 mm. in height. These cylinders were separated by a thin glass plate. A cork closed the lower opening of the cylindrical vessel and through this passed the glass tubes provided with stop cocks, by means of which the vessel could be filled with any gas at any pressure. A thermometer was inserted through a tubulature about 50 mm. below the thin glass plate placed in a horizontal position. The upper vessel was filled with boiling water, which was kept in ebullition during the experiment by the introduction of steam. To guard against radiation the apparatus was placed in a beaker and this beaker in another filled with water of a constant temperature. Magnus found with this apparatus that the heat rays are partially absorbed by their passage through air. With hydrogen, however, he found that the temperature increased with the density, and even at ordinary atmospheric pressure it had greater diathermancy than a vacuum. He accordingly concluded that hydrogen was similar to the metals in regard to conducting power. Dr. Buff's

apparatus was similar to that of Magnus, with the exception that a brass cylinder was cemented upon the glass vessel; instead of the thin glass plate a polished metallic surface, constituting the bottom of the brass vessel, faced the enclosed thermometer, a double wall surrounded the cylinder filled with cotton wool to prevent too rapid cooling. The glass cylinder was 20 cm. high and 7·5 cm. in diameter—its lower edge was ground so as to fit air-tight upon the plate of an air pump. The thermometer in Magnus' apparatus was superseded by a thermo-electric one formed of flattened German silver and iron wires soldered together. This junction is placed very near the bottom of the brass cylinder and the leading wires were conducted through the plate of the air pump. The whole apparatus was surrounded by a wider glass cylinder which also fitted upon the plate of the air pump and the enclosed space could be filled with water. With this apparatus, Buff discusses the controversy between Magnus and Tyndall in regard to the diathermancy of moist air. Although his experiments are not completed, Buff finds that his results agree pretty closely with those of Magnus, and cannot believe with Tyndall that the thermal absorption of moist air is from twenty to forty times greater than dry air. He sums up his results as follows:—

(1.) "The thermal conductivity of hydrogen and of other gases is far too small to admit of its being proved by the method Magnus adopted. The assumption that the conductivity of hydrogen is similar to that of the metals, if by this statement anything more is meant than that hydrogen, like solid and liquid bodies, is capable of transmitting heat from molecule to molecule, is therefore not justified.

(2.) On the other hand, hydrogen possesses a diathermancy closely approaching that of a vacuum.

(3.) Dry air absorbs from fifty to sixty per cent of the rays of heat which it receives from a source heated to the boiling point of water.

(4.) The absorptive power of moist air surpasses that of dry air by a trifling percentage, but by no means to such a degree as hitherto had been assumed by several physicists.

(5.) Rock salt is not absolutely diathermanous for the so-called dark rays of heat; its thermal color rather resembles that of dry air."—*Phil. Mag.*, Dec. 1877, page 401. J. T.

11. *Spectrum of the Electric Spark in compressed gases.*—CAZIN and WÜLLNER have been experimenting separately upon the subject. Cazin expresses his results as follows: The electric spark resembles an ordinary gas flame. In both sources of light there are, beside the peculiar vaporous particles which give line spectra, other solid and liquid particles which give a continuous spectrum. The admixture of the last which arises from the character of the electrodes and sides of the containing vessel increases with the pressure, so that finally the line spectrum upon the higher continuous spectrum disappears. In the so-called aureole the mate-

rial particles are wanting, and the aureole can be compared to the blue basis of the ordinary gas flame. Cazin enclosed nitrogen in a species of quicksilver pizometer; the electrodes were of platinum. Beginning with two atmospheres, the channelled spaces and all the nitrogen lines save six disappeared. At ten atmospheres only the lines $\lambda=567$ and $\lambda=500$, together with a very bright line $\lambda=424$, which first appeared under five atmospheres, and was attributed by Cazin to nitrogen, remained. These lines which were not perceptible at ordinary pressures, together with the always present sodium lines and a platinum line which appeared at fifteen atmospheres of pressure, remained up to forty atmospheres. These results needed to be confirmed by the check of photography, and Herr Cazin has already obtained interesting confirmations by this process. The above results were obtained by passing the spark through the vapor of hyponitrous acid at a pressure of thirty atmospheres. Herr Wüllner objects to the conclusions of Cazin that the continuous spectrum is due to the broadening of the bright lines. According to Wüllner this is only the case for the hydrogen and a portion of the oxygen spectrum. The phenomenon noticed by Cazin in the nitrogen spectrum was due to admixtures of carbon compounds, especially the carbonates. Wüllner has obtained a third order of spectra from nitrogen, and he attributes Cazin's results to the fact that by increasing the pressure he was able to pass from one spectrum to another.—*Beiblätter Physik and Chemie*, No. 11, p. 620. J. T.

II. GEOLOGY AND MINERALOGY.

1. *Silurian Plants;* by LEO LESQUEREUX. 12 pp. 8vo. From the Proceedings of the Amer. Phil. Soc., Oct. 19, 1877.—Mr. Lesquereux has here published, with a plate for illustration, his latest facts respecting the land plants of the Ohio Lower Silurian, first announced by him in this Journal for January, 1874 (p. 31). The recent report by Count Saporta of a discovery of a fern from the Silurian slates of Angers, France, probably of the age of the Cincinnati group, first brought out in 1876, gives additional importance and interest to the earlier discoveries in Ohio. Mr. Lesquereux calls the species he first described *Protostigma sigillaroides;* and adds now, from the same rocks—the Cincinnati group, near Cincinnati, *Sphenophyllum primævum* Lesqx., *Psilophytum gracillimum* Lesqx. (near Covington, Ky., opposite Cincinnati).

In this paper Mr. Lesquereux also describes a Fungus (*Rhizomorpha sigillariæ* Lesqx.), found in connection with a Sigillaria in cannel coal at Cannelton, Beaver Co., Kentucky.

2. *Modified drift in New Hampshire;* by WARREN UPHAM, 176 pp. roy. 8vo. From the third volume of the final Report of the Geology of New Hampshire. Concord, N. H. 1877.—Mr. Upham's memoir treats of the drift deposits, more or less stratified, which occur along the river valleys, about the lakes, and on the sea shores of New Hampshire. It is the result of a careful

study of these Quaternary deposits, carried on while acting as assistant geologist in the survey of the State. The conclusions here brought out with regard to the long gravel deposits of the larger valleys, which had been called kames, or eskers, are given at length in the last volume of this Journal, in an article contributed by himself. All parts of the subject are worked up with thoroughness and the facts are given with full details in the volume. The memoir is well illustrated by maps and sections.

3. *Ueber das Krystallsystem und die Winkel des Glimmers;* von N. v. KOKSCHAROW.—The monograph of the eminent Russian mineralogist upon the crystallization of the micas covers some eighty-five pages quarto, including the results of a long series of accurate observations. The principal conclusions arrived at are as follows:—That all the micas, without exception, belong to the orthorhombic system, with monoclinic habit; that with them all the plane angle of the base (cleavage plane), and also that of the fundamental prism is exactly 120°, consequently when the acute edges are truncated the prism is geometrically a hexagonal one. The ratio of the axes is c (vertical) : $\bar{b} : \breve{a} = 2·84953 : 1·73205 : 1$. The author gives also a list of the crystalline planes observed by him and others, and a discussion of the methods of twinning.—*Memoirs of the Imperial Academy of St. Petersburg*, May 7, 1877.

E. S. D.

4. *Die Glimmergruppe; 1 Theil, von G. Tschermak.*—The memoir by Prof. Tschermak upon the mica family, of which the first part has been received, has been long promised, and is of especial interest in view of the earlier monograph mentioned above. In the present portion of the memoir, the author gives in detail his crystallographical and optical examination of the different species of the mica family, reserving the discussion of their chemical relations to a second paper. The exact determination of their optical characters has enabled the author to prove that all the micas, although a variation in angle from the orthorhombic form may not be established, are nevertheless *monoclinic.*

The micas are divided into two groups; with the first the plane of the optic ages is perpendicular to the plane of symmetry and with the second is parallel to it; they are as follows:—

	I.	II.
Biotites:—	Anomite.	Meroxene, Lepidomelane.
Phlogopites:—		Phlogopite, Zinnwaldite.
Muscovites:—	Lepidolite. Muscovite. Paragonite.	
Margarites:—	Margarite.	

The name *Meroxene*, first introduced by Breithaupt, is employed by the author to include all the magnesia micas of Vesuvius, and also all other magnesia micas closely related to them and not falling into the other divisions. On the other hand the magnesia micas which fall into the second class as defined above are called *anomite* (Gr. *ανομέω*); in this class falls the mica of L. Baikal, and that of Greenwood Furnace, N. Y.—*Vienna Academy.*

E. S. D.

III. BOTANY AND ZOOLOGY.

1. *The Hybridization of Lilies;* by FRANCIS PARKMAN.—In No. 15 of the second volume of the *Bulletin of the Bussey Institution*, under the above title, Mr. Parkman gives a summary of his experiments, during ten or twelve years, in crossing Lilies. One of the earlier results, and that which the horticulturists count as the eminent one, was the production of that magnificent hybrid between *L. auratum* and *L. speciosum*, with flower resembling the former in fragrance and form and the most brilliant varieties of the latter in color, which was brought out in England under the name of *Lilium Parkmanni.* The interesting physiological point which Mr. Parkman here records is, that this striking novelty was wholly unique; that all the other seeds of the same parentage which germinated, over fifty in number, gave rise to plants which in the blossom showed no trace of the male parent, *L. auratum* but were exactly like the female parent, *L. speciosum.* That these plants were truly hybrids, notwithstanding,. is well made out, 1, by the precautions taken against any possible access of own pollen; 2, by the scantiness of seed, most of which was abortive; 3, "such good seed as there was differed in appearance from the seed of the same lily fertilized by the pollen of its own species," which is smooth, while this was rough and wrinkled, and 4, the stems were mottled after the manner of the male parent.

It would naturally be thought that this slight but evident impression of the character of the male parent might be deepened by iteration. That was tried next year, when the flowers of several of these plants were fertilized with the pollen of *L. auratum* precisely as their female parent had been fertilized. The result was an extremely scanty crop of seed, "but there was enough to produce 8 or 10 young bulbs. Of these, when they bloomed, one bore a flower combining the features of both parents, but though large, it was far inferior to *L. Parkmanni* in form and color; the remaining flowers were not distinguishable from those of the pure *L. speciosum.*" The article records the results of various similar attempts to hybridize other lilies. For instance, our *L. superbum* was pollenized with eight different old-world species. The result was, that capsules, apparently perfect, were abundantly produced; some of them contained nothing but chaff, others had a few imperfect seeds, still others gave a fair supply of good seed. From this seed several hundred young bulbs were produced. "But when these came into bloom, not a single flower of them all was in the least distinguishable from the pure *L. superbum.*" Moreover, in this case (different from the other) "not one of the eight different male parents had imposed his features on his hybrid offspring. Not only in their flowers, but in their leaves, stems and bulbs, the young plants showed no variation from their maternal parent." The experiment proceeded one generation farther. "In the following year I set some of them apart from the rest, and applied to them, as to their mother before them, the pollen of several species of lilies. This time the seeds were extremely scanty. A few, however, were produced; but the plants and flowers that resulted from them were, to all appearance, *L. superbum* pure and simple."

In trials of other species results intermediate between these two cases were obtained. For instance the pure white of the perianth of *L. longiflorum* came out unstained in the progeny raised by a crossing with *L. speciosum;* and the herbage was equally unaffected; but in that or the next generation "distinct evidence could be seen of the action of alien pollen" in the changed color of many of the anthers, and in the abortion of others. They also showed differences of habit among themselves, some being very tall and vigorous, and others compact and bushy, with a tendency to bloom in clusters; but these may have been mere seedling variations, with which the hybridization had nothing to do." Yet some of these marks correspond with known results of hybridization.

That offspring should partake unequally of the characters of the two parents is a matter of common observation. That in the genus *Lilium* the hybrid offspring should in forty instances out of fifty take almost all its traits from the female parent, as Mr. Parkman has shown, is very remarkable. That, in not a few instances, it should take them all, so far as can be seen—that the paternal influence should be represented by zero—is most extraordinary. If parthenogenesis in plants were more unequivocally demonstrated, so as to be placed in certain instances quite beyond doubt (which is hardly the case), then we should regard the supposition which Mr. Parkman mentions as having been suggested to him, viz: that in the case of *L. superbum* the embryo was developed without male influence, to be quite as likely as the alternative of the progeny's inheriting everything from the female and nothing from the male parent; in fact the two suppositions approximate to the same thing. We are supposing the total absence of male parent's characters, and also that the alternative of fertilization by chance pollen of the species is absolutely excluded. Of this there is very high probability, yet not entire certainty. One of Mr. Parkman's "reasons for believing that parthenogenesis had nothing to do with the cases in question," viz., that some of the lilies were young plants that never had bloomed before, has no application, but comes from a slight confusion of the idea of parthenogenesis with the effect in some animals of a previous male influence upon next succeeding progeny, which is quite a different thing.

The fact that more than one sort of hybrid may be generated between the same two species, copulated in the same way, must do away with the old mode of naming hybrids by a combination of the name of the two parents, that of the male preceding. The plan had the double advantage of indicating the origin of the cross, and of distinguishing hybrids from species in nomenclature; but in practice it proves insufficient. A. G.

2. *Thuret's Garden.*—Upon the death of M. Thuret and the sale of his place at Antibes, it was feared that his noble and unsurpassed collection of living plants, of special botanical interest, would be broken up. But it is now stated (Gard. Chron., Dec. 15) that, "thanks to the generosity of Madame Louise Fould, it will for the future be devoted to public uses in connection with the Jardin des Plantes at Paris." An adjunct of this sort in Mediterranean climate is just what the Jardin des Plantes needs; and this ensures not only the preservation, but the increase and the scientific usefulness of a very rich collection of warm-temperate plants of all countries, specially those of dry regions. A. G.

3. *Dr. Engelmann's new botanical Papers in the Transactions of the Acad. Sci. St. Louis*, vol. iii, Nov.-Dec., 1877.—The most important of these papers is an appendix to that on The Oaks of the United States, read in the spring of 1876, and published soon afterward. A full notice of it appeared in this Journal. The continuation, of as many pages (21), is equally worthy of particular notice; but it is more difficult to give an abstract of it, and the space is wanting. Certain corrections are made, an improved classified enumeration of our species is given; then follow "additional notes," a continuation of these, and finally more additional notes. We have not yet the last word; but thoroughly conscientious work of this kind is most valuable. Now that the Oaks are comparatively clear, we look to Dr. Engelmann for the elaboration of our *Coniferæ*. A good contribution is made in the second paper before us: "*The American Junipers of the section Sabina*, which fills ten pages with the discussion of our nine species, Mexican and West Indian being included. The third paper is a small one on The Flowering of *Agave Shawii*, with a plate illustrating floral details. It must suffice merely to announce these publications, which are indispensable to working botanists. A. G.

4. *A new range for two Orchids* is given by the Rev. Dr. Wibbe of Oswego, New York, who sends *Listera australis* and *Habenaria leucophlæa*, gathered by him in "Lily Marsh," nine miles east of Oswego. The first was not known north of the pine barrens of New Jersey, and is a southern plant. The second belongs to the district from central Ohio west, but Mr. Hankenson had already detected it in Wayne Co., in the western part of New York. Pastor Wibbe also sends a polymerous state of *Trillium erythrocarpum*. Something of the kind not rarely occurs in *Trillium*. This plant, which has been constant since discovered five years ago, has all the parts from leaves to carpels regularly increased (in the leaves apparently by chorisis) from three to nine, except that the stamens hardly keep up to double the number of the petals. A. G.

5. *Botanische Untersuchungen über Schimmelpilze.* Part III. *Basidiomycetes;* by Dr. Oscar Brefeld. Leipsic, 1877. 4to. —In this volume of over two hundred pages Brefeld gives a detailed account of his cultures of different *Basidiomycetes*. The work is full of interesting facts and observations and is excel-

lently illustrated. Had it been shortened by one-half, it would have been easier reading, certainly for foreigners. As it is, it is too detailed to commend itself to the general reader. The greater part of the book is devoted to species of *Coprinus*, especially *C. stercorarius*. The spores of this species which were sown in decoctions of horse dung germinated readily and produced first a mycelium and then a pileus and spores. In many cases a sclerotium was first produced from which grew later the pileus. In some species of *Coprinus*, but not in all, conidia (stäbchen) were produced. The object of Brefeld's cultures was to ascertain the existence of male and female organs in the Basidiomycetes. It had been stated by Rees and Van Tieghem that the "stäbchen" were male organs. Van Tieghem afterwards, however, changed his mind and concluded that they were conidia. Brefeld denies entirely that they are male organs and maintains that the pileus and spores of Coprinus are produced directly from the mycelium by a vegetative process without the intervention of any sexual organs. He arrives at this conclusion by the following process. He has examined the mycelium, the sclerotium, and all parts of the fruit-bearing body and finds no trace of sexual organs. But as the plants examined were complex, and it was possible that he had overlooked something, he goes farther and makes slices of the sclerotium, the stipe, and the pileus and places them in conditions favorable to farther growth. Now, if the fruit-bearing body is produced by a sexual action which takes place in the sclerotium, when the sclerotium is cut into several pieces only those pieces which contain the sexual organs can produce fruit-bearing bodies. The same is true of cutting the stipe and pileus. Brefeld found that, however he sectioned the organs mentioned, the hyphæ of the cut surface grew up and formed one or more new fruit-bodies. Hence there can be no sexual organs either in the sclerotium, stipe or pileus. The mycelium and the hymenium in *Coprinus* are comparatively simple and it is impossible to recognize in them any sexual organs according to Brefeld. The latter part of the book is devoted to a consideration of some other *Basidiomycetes*, especially *Amanita* and *Agaricus melleus*. The last named species which is several inches high was raised by Brefeld from the spore in his cultures and he confirms the view of Hartig that the so-called *Rhizomorpha subcorticalis* is only the sclerotioid state of this fungus. Brefield considers that *Rhizomorpha subterranea* is only an underground form of *R. subcorticalis*. In concluding, he gives some general views with regard to the Basidiomycetes and a table showing the relationship and descent of the different groups. Some of his views are novel, as, for instance, that the teleutospore of the *Uredinei* is of the nature of a resting-spore and that the promycelium and sporidia represent the basidium and spores of the *Hymenomycetes* as shown in the simplest form in some of the *Tremellini*. In this work we see about the first attempt to refer different species of fungi to a hypothetical type which was the common ancestor. This method which has been used with such advantage by zoologists is not likely to answer as well in fungi of which almost no definite fossil remains exist. W. G. F.

5. *Beiträge zur Entwickelungsgeschichte der Flechten.* Part II. *Ueber die Bedeutung der Hymenialgonidien;* by Dr. E. STAHL. Leipsic, 1877. 8°.—An admirable essay, excellently planned, and beautifully written. In the first part of this work which has already been noticed in the Journal, Stahl gave an account of the sexual organs of lichens. In the present, he considers the signification of the hymenial gonidia of course in its bearings on the Schwendener theory that lichens are Ascomycetes parasitic on algæ. The species studied were *Endocarpon pusillum*, *Thelidium minutulum*, and *Polyblastia rugulosa.* Stahl considers that the hymenial gonidia are derived from the thalline gonidia and present a different aspect simply from their different surroundings. When the spores are discharged some of the hymenial gonidia are always discharged with them. As the spores germinate, their hyphæ fasten themselves upon the gonidia which then increase more rapidly than before. In from four to five months Stahl succeeded in raising new perithecia and spores by his culture of the spores of *Endocarpon* with the hymenial gonidia. In like manner, he raised perithecia and spores of *Thelidium* from culture of the spores with the hymenial gonidia. He went still farther. Some of the spores of *Endocarpon* together with the hymenial gonidia which had been discharged with them were placed in water in which the gonidia became diffused. Then spores of *Thelidium* were placed in the water and on germinating they attached themselves to the gonidia of *Endocarpon.* By such a proceeding Stahl was able to produce a thallus of *Thamnidium* with gonidia of *Endocarpon.* It will be seen that Stahl's experiments have a most important bearing in favor of the Schwendener theory. To Schwendener and Bornet we are indebted for the most accurate account of the anatomy of the thallus of lichens, but hitherto no one has been able to reproduce the fruit of a lichen by cultivating the spores with the gonidia. Bornet and Rees succeeded in making the hyphæ of certain *Collemaceæ* grow in *Nostocs*, but it was maintained by the opponents of the algo-fungal theory that the union of hyphæ with a *Nostoc* did not constitute a true *Collema* thallus because, as there was no fruit formed, it showed that the combination was not normal. By producing the lichen-fruit in his cultures, Stahl removes this objection. In constructing a thallus of one species by letting its spores germinate with the gonidia of another, he proves that the gonidia are neither produced by the hyphæ nor is there any essential connection between them except that of a parasitism. We notice the statement in the number of Just's Jahresbericht that has just appeared that the lichens will not be hereafter kept as a distinct group apart from algæ and fungi, but will appear as a subdivision of *Ascomycetes.* W. G. F.

7. *Acetabularia Mediterranea;* by A. DE BARY and E. STRASBURGER. Ext. Bot. Zeit., Nov. 1877.—In this paper the development of *Acetabularia* is given partly by De Bary who cultivated

spores received from Antibes in his laboratory at Halle and partly by Strasburger who had studied the species in the Mediterranean. The anatomy and formation of the spores had been previously published by Woronin in the Annales des Sciences. To this De Bary adds an account of a peculiar process connected with the base of the plant. The spores produce a number of zoöspores which conjugate by twos or by larger numbers. Zoöspores from the same sporangium do not conjugate with one another but only with those coming from another sporangium. W. G. F.

8. *Entwickelungsgeschichte des Prothalliums von Gymnogramme leptophylla;* by Dr. KARL GOEBEL. Inaugural Dissertation. Strasbourg, 1877.—A detailed account of the growth of the prothallus of *Gymnogramme leptophylla.* The antheridia are like those of *Aneimia hirta.* The archegonia originate in a peculiar process, which projects from the under side of the prothallus. If the archegonia are not fertilized, the process grows into a new prothallus. The prothallus is also propagated by means of a kind of adventitious buds so that a single prothallus may live for several months and attain a considerable size. W. G. F.

9. *On a new Species of Parasitic Green Alga belonging to the genus Chlorochytrium of Cohn;* and *On a species of Rhizophydium parasitic on Species of Ectocarpus, with notes on the Fructification of the Ectocarpi;* by Prof. EDWARD PERCEVAL WRIGHT. Dublin Trans. R. I. Acad., vol. xxvi.—The species of *Chlorochytrium* was found by Prof. Wright on several plants, among others on *Calothrix confervicola,* where it was probably seen by Harvey and mistaken by him for the spores of that plant. The *Rhizophydium* as shown by Prof. Wright's plate seems to be the parasite which is very common on *Ectocarpi* of our own coast. At the end of the second article is a consideration of the different forms of sporangia found in British species of *Ectocarpus.* W. G. F.

10. *Transpiration in Plants.*—In the last number of this Journal, attention was called to a recent paper by Wiesner, showing that the rays of light which correspond to the absorption bands of the chlorophyll spectrum, are the most efficient in transpiration. Dehérain (in Ann. des Sci. Nat. for Sept., 1877) reviews Wiesner's communication and states that in 1869 he arrived, by a different method of experimenting, at an opposite result, namely, that yellow light is more active than blue in transpiration. Still claiming that his method is free from errors, he submits the following explanation of the contradiction: the rays which determine transpiration are the ones absorbed by chlorophyll, but they effect transpiration proportionately to their own convertible energy; it may therefore be supposed that a yellow ray, richer in calorific radiation, can act with greater energy, although only partly absorbed by chlorophyll, than can a blue ray, poor in calorific radiations, which is more completely absorbed. G. L. G.

11. *On Japanese Lingula and Shell Mounds.*—At a meeting of the Boston Society of Natural History, December 19, Professor EDWARD S. MORSE communicated some of the results of his work

in Japan. His main object in visiting Japan was to study more fully a group of animals upon which he has been at work for a long time—the Brachiopoda.

Accepting an appointment as professor of Zoology in the Imperial University of Tokio he established a Zoological station on the coast for the purpose of collecting material for the University Museum and for the training of Japanese assistants in the work.

His studies of Lingula have brought out many points new to science. The discovery of auditory capsules in the class of Brachiopods is one of the most important. These organs he determined in a species of Lingula and their position and general appearance recall the auditory capsules as figured by Claparède in certain tubicolous Annelids. He has also cleared up many of the obscure points in regard to the circulation, and is prepared to maintain the absence of anything like a pulsatory organ, the circulation being entirely due to ciliary action. Mr. Morse also described some of the habits of Lingula. While partially buried in the sand the anterior border of the pallial membranes contract in such a way as to leave three large oval openings, one in the center and one on each side. The bristles which are quite long in this region of the animal arrange themselves in such a way as to continue these openings into funnels and entangle the mucous which escapes from the animal; these funnels have firm walls. A continual current is seen passing down the side funnels and escaping by the central one.

They bury themselves very quickly in the sand, and the peduncle agglutinates a sand tube. They attach themselves by means of this tube to the bottom of dishes in which they are confined.

Mr. Morse exhibited living specimens of Lingula which he had brought from Japan in a small glass jar. The water had only been changed twice since August 20th, and yet no specimen had died. This illustrated more fully the vitality of Lingula than the experiments he had made on the North Carolina Lingula several years since.

A description was also given of an ancient shell-mound discovered by Professor Morse at Omori near Tokio, and photographs of many of the vessels exhumed were exhibited. The general aspects of the deposit were like those described by Steenstrup in Denmark, and by Wyman, Putnam and others on the United States coast. The implements were mostly horn. Only three rude stone implements were discovered. The pottery was remarkable in showing a great variety of ornamentation, though it was very rude in character and did not exhibit that finish seen in ancient Corean pottery which is found in the Empire. In the incised character of the markings it recalls the pottery of the east coast of the United States.

In the character of the raised knobs for handles on the edge of the vessels it shows the closest resemblance to pottery discovered by Professor Hartt in Brazil. Mr. Morse was not prepared to say whether it was early Aino or a race which preceded the Ainos and which the Ainos displaced in their occupation of the island from the north.

12. *The American Naturalist.*—The January number of this scientific monthly comes to us from Philadelphia, where this Journal is to be published hereafter by Messrs. McCalla & Stavely. Professor E. D. Cope is now associated with Professor Packard in the editorial management of the journal. It has always been successful in combining the scientific and popular in its articles, and has contributed greatly to science-education in the country as well as to the progress of science; and the Prospectus states that this will be still its aim. It is also announced that the department of birds will be edited by Dr. Coues, and that of microscopy by Dr. R. H. Ward; and that Professor O. T. Mason will continue his monthly summaries of anthropological news.

13. *Bulletin of the Nuttall Ornithological Club.* 48 pp. 8vo. Cambridge, Mass.—The third volume of this Quarterly Journal of Ornithology commenced with this year. The January number contains several valuable papers on birds and a very full selection of notes, besides extracts from the recent literature of the science. While thoroughly scientific, most of its articles have a popular interest, making it an attractive journal for the amateur as well as the man of science. It has for its frontispiece a beautiful colored plate of Baird's Bunting,—*Passerculus Bairdi* of Coues,—illustrating a paper by Dr. Elliot Coues, U. S. A. Professor J. A. Allen has a paper on *An inadequate "Theory of birds' nests,"* in which, after stating various facts, he expresses the following conclusion: "The most surprising thing about Mr. Wallace's 'theory of birds' nests' is its inadequacy and its irrelevancy to the facts it was proposed to explain; and in this respect it was scarcely excelled by any of the crude inventions into which the more ardent supporters of the theory of evolution by means of what has been termed 'natural selection' have been betrayed." The charge for the Bulletin is only two dollars per year.

15. *Nests of "the Gardener."*—In the Annali di Storia naturale del Museo Civico di Genova, the illustrious traveller and botanist, Prof. O. Beccari, describes the wonderful gallery or bower-constructions of the *Amblyornis inornata*, observed by himself in the Arfak Mountains. The huts and gardens, as built and laid out by this bird, which is called "the gardener," seem to surpass any production of intelligence and taste for the beautiful hitherto described and observed in birds of the Paradise family.—*Nature*, Dec. 6, p. 110.

IV. Astronomy.

1. *Meteors observed in Cambridge, Mass., November* 3, 1877. —Meteors were seen on this night at the following Cambridge times: $6^h\ 53^m$; $9^h\ 47^m$; $10^h\ 47^m$; $11^h\ 7^m$. They proceeded from the same radiant, which was near to *Mars*, and their directions were towards the S.W., all conformable to the above radiant.

C. H. B. AND L. H.

2. *Prize for the discovery of Comets.*—The Imperial Academy of Sciences at Vienna has resolved to continue, until further notice, the prizes, awarded since 1872, for the discovery of telescopic comets. The awarding of such a prize, consisting, according to the wish of the receiver, either in a gold medal or in its money

value of twenty Austrian ducats, is connected with the following conditions:

(1.) Prizes are awarded only for the first eight successful discoveries of each calendar year; for comets that, at the time of their discovery, were telescopic, i. e., invisible to the naked eye, that had not been seen before by any other observer, and the appearance of which could not have been predicted. The priority is to be decided by the epoch of the first position. (2.) The discovery is to be made known to the Imperial Academy of Sciences, immediately, and without waiting for further observations, by telegraph where practicable, otherwise by the earliest mail. The Academy will communicate the news without delay to several observatories. (3.) The first notice must contain as accurately as it is possible to the observer the position and motion of the comet, besides place and time of the discovery. This first notice is to be supplemented at the next occasion by later observations. (4.) If the comet should not have been verified by other observers, the prize will be awarded only when the observations of the discoverer are sufficient for determining the orbit. (5.) The prizes will be awarded in the general session held at the end of May of each year. If the first notice of the discovery arrives between the first March and the last May, the prize will be decided in the general May session of the Academy in the next year. (6.) Application for the prize is to be made within three months after the first news of the discovery has arrived at the Imperial Academy. Later applications will not be considered. (7.) The astronomers of the observatory of the University at Vienna will be judges, whether the conditions contained in arts. 1, 3 and 4 have been fulfilled.

3. *Index Catalogue of Books and Memoirs relating to Nebulæ and Clusters, &c.;* by EDWARD S. HOLDEN. Smithsonian Institution, Washington, 1877. 8°, pp. ix and 111.—About half of this valuable work of Professor Holden is devoted to the catalogue named in the title. The rest consists of: a list of books and memoirs relating to the nebula in Orion; a similar list of those relating to variable nebulæ; a list of drawings of nebulæ; and two indices to Sir Wm. Herschel's and Messier's Catalogues of Nebulæ and Clusters.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Telephone in England.*—Col. W. H. REYNOLDS has concluded a contract with the English Government by which the Post Office Department has adopted the Bell telephone as a part of its telegraphic system. In a recent telephonic experiment in connection with the cable 21¾ miles long, between Dover and Calais, there was not the slightest failure during a period of two hours. Though three other wires were busy at the same time, every word was heard through the telephone, and individual voices were distinguished. This important experiment was conducted by Mr. J. Bourdeaux, of the Submarine Telegraph Company. Some very successful experiments were made with the telephone on Saturday

night between Aberdeen and Inverness, a distance of 108 miles. Songs and choruses were distinctly transmitted, and conversation was carried on at times with marvellous distinctness, notwithstanding the weather was unfavorable. The experiments were made with Professor Bell's instruments. The Berlin correspondent of the Daily News states that a Berlin house is making a number of telephones for experimental use in the Russian army. The result is awaited with great curiosity in military circles. The Cologne Gazette denies that any telephone is in existence between Varzin and Bismarck's office at Berlin. Our contemporary says that the distance, 363 kilometers, is too large for using a telephone with any advantage.—*Nature*, Dec. 6, p. 109.

2. *A Manual of Heating and Ventilation in their practical application for the use of Engineers and Architects, etc.;* by F. Schumann, C.E., U. S. Treasury Department. 89 pp. 8vo. New York, 1877. (D. Van Nostrand.)—This little book contains the formulas and data required by architects and engineers in putting to practice in the construction of buildings, the theoretical principles of heating and ventilation. The subject is systematically treated, and all the explanations are given that are required for an intelligent use of the formulas.

3. *Mesmerism, Spiritualism, etc., historically and scientifically considered;* being two Lectures delivered at the London Institution, with Preface and Appendix; by Wm. B. Carpenter, C.B., F.R.S. 158 pp. 12mo. New York, 1877. (D. Appleton & Co.) —The learned physiologist of London, Dr. Carpenter, has shown good sense, thorough knowledge and excellent judgment in these lectures.

4. *The Telephone;* an account of the phenomena of Electricity, Magnetism, and Sound, as involved in its action, with directions for making a speaking Telephone by Prof. A. E. Dolbear. 128 pp. 12mo. Boston, 1877 (Lee & Shepard).—This is a clear and interesting account of a subject which is exciting much interest at the present time.

5. Auguste de la Rive.—The number of the Bibliothèque Universelle (Archives des Sci. Phys. et Nat.) for September, 1877, has its 254 pages occupied with a biographical notice of the eminent Swiss Physicist, Auguste de la Rive, who died, at the age of seventy-two, on the 27th of November, 1873.

Proteus, or Unity in Nature, by Charles Bland Radcliffe, M.D. 214 pp. 8vo. London. (MacMillan & Co.)

The Physiology of Mind. By Henry Maudsley, M.D. 544 pp. 8vo. New York. (D. Appleton & Co.)

Meteorological Researches. By William Ferrel. Part I. On the Mechanics and the General Motions of the Atmosphere. 50 pp. 4to. With 6 charts. U. S. Coast Survey, C. P. Patterson, Superintendent. Washington. 1877. An important memoir discussing mathematically, from meteorological data with reference to the whole earth's surface.

OBITUARY.

Ruhmkorff died in Paris on the 20th of December at the age of seventy-four. He gave his first "Ruhmkorff Coil" to the world in 1851, and received for it from the French Exhibition in 1855 the first prize of 50,000 francs.

THE

AMERICAN

RNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

XXI.—*Coggia's Comet—its Physical Condition and Struc-*
. Physical Theory of Comets; by Prof. W. A. NORTON.

GIA'S Comet (Comet III, 1874), the last that was con- usly visible to the naked eye, was attentively observed at atories in both hemispheres. The reported results of the e series of observations and determinations made with escope, spectroscope, and polariscope, appear to reveal the ial features of its physical condition and structure, and to important indications with regard to the probable phys- eory of comets in general. The following are the ele- of the parabolic orbit of the comet, computed by M. hof (Astr. Nachr., No. 2003). T=1874, July 8·85664, T., $\Pi=271°\ 6'\ 19''\cdot5$, $\Omega=118°\ 44'\ 25''\cdot3$, $i=66°\ 20'$ log. $q=9\cdot829826$, which gives, per. dist. $q=0\cdot67581$. n direct.

ure and Condition of the Cometic Matter.—The following e general results of the observations made with the spec- pe and polariscope* with the view of ascertaining the and condition of the matter of the comet; in the nucleus ma or envelopes, and as more widely diffused in the tail.

The light of the tail and coma was partially polarized in e through the axis of the tail.

The spectrum of the comet consisted of three or more bands on a continuous spectrum. This continuous spec- was faint on July 7, but became much brighter by July The three bright bands were identical with those obtained ssing a spark from an induction coil through gaseous-

nth. Not. of Royal Astr. Soc., 1873–4, pp. 489 to 491; 1874–5, p. 83. achr., No. 2018, pp. 18 to 32.

dioxide of carbon (carbonic acid gas). Other experiments have shown that the volatile hydro-carbons give, with the electric spark, the same spectroscopic bands; and that these are wholly due to the momentary incandescence of the carbon molecules of the compound. Several other comets have given the same "carbon bands." Brorsen's comet, a faint circular nebula, invisible to the naked eye, proved to be an exception. Three bright bands were observed in its spectrum, but they differed in position and other features from the carbon bands.

(3.) "The spectrum of the nucleus was continuous, but it appeared to have traces of numerous bright bands, and three or four dark lines also were seen."

(4.) The light from the tail gave a continuous spectrum, without superimposed bright bands.

From these results we may draw the following conclusions:

(1.) From the bright bands observed we may infer that the coma consisted in a large measure of matter in the gaseous state; and that the actual gaseous substance was either dioxide of carbon (i. e. carbonic acid gas), or oxide of carbon, or the vapor of some hydro-carbon.

(2.) The light of incandescence of the gaseous particles, which furnished the bands, must have been of electric origin; since the heat of the sun could not have been sufficient to ignite the most inflammable vapor.

(3.) The continuous spectrum on which the three carbon bands were seen, affords no decisive evidence of the presence in the coma of discrete solid particles, since it may have resulted from the solar light reflected from the gaseous particles. Such light would not have been sufficiently intense to give the dark solar lines.

(4.) The "traces of bright bands" seen in the spectrum of the nucleus reveal the presence of vapors at its surface shining by electric light. The bright continuous spectrum may have been wholly due to reflected solar light (since dark solar lines were not wanting), or partly to discrete solid particles rendered luminous by electric discharges. The light reflected from the solid nucleus, or from dense vapors or clouds near its surface, may well have been of sufficient intensity to make the gaseous carbon bands resulting from electric discharges inconspicuous.

(5.) The spectroscope did not give any decisive evidence with regard to the state of the matter in the tail—whether gaseous or composed more or less of discrete solid particles; but since the tail was formed of matter flowing in continuous streams from the head, we must suppose that it was made up chiefly of gaseous particles, like the head.

(6.) The light of the tail was exclusively reflected solar light.

The experiments of Professor Arthur W. Wright, of Yale College, on the gases from stony meteorites,* have furnished strong evidence in support of the hypothesis first propounded by him, that the cometic substance is gaseous carbon dioxide. He found that "in meteorites of the stony kind, the characteristic gas is carbon dioxide, and this, with a small proportion of carbonic oxide (oxide of carbon), makes up more than nine-tenths of the gas given off at the temperature of boiling water, and about half that evolved at a low red heat." The spectrum of the gases, obtained by passing an electric spark through a small tube containing the gases at a low tension, consisted of the hydrogen and carbon spectra together. The three bright bands of the carbon spectrum were coincident in position with those in the spectra of comets, and had the same relative order of intensity. The close relationship now known to subsist between comets and meteors renders it highly probable, as suggested by Professor Wright, that the cometic matter is identical with the gaseous matter found associated with stony meteorites, and consists chiefly of carbon dioxide disengaged from the nucleus of the comet by the heat of the sun.

If we adopt this hypothesis with regard to the nature and origin of the cometic substance, the question arises in what condition does the carbon dioxide exist, in its association with the matter of the nucleus? We may at once admit, with Professor Wright, the high probability that it has become intimately associated with it by interpenetration, or by surface concentration upon small metallic grains, as it is found in meteoric stones. But it is also to be observed, that at the temperature of free space, which must prevail on most comets throughout the greater part of their periods of revolution, a large accumulation of carbon-dioxide might exist on their surface in the solid condition, or as a layer of detached solid particles. This may be inferred from the results of Faraday's experiments on the condensation of this gas into a liquid at extremely low temperatures. He found at the temperature of 80° C., below the freezing point, a pressure of one atmosphere sufficed to condense it into a liquid. 80° C. is, according to Fourier, 20° C. below the temperature of free space, but, according to the more reliable determination of Pouillett 60° C. above it. At this temperature, −140° C., a small fraction of one atmosphere of gaseous pressure should suffice to condense the gas into a liquid.

Now, when a comet, supposed to have an accumulation of solid carbonic acid at the surface of the nucleus, is in the act of approaching the sun, the increasing amount of heat received from the sun should give rise to copious evolutions of the carbon-dioxide in the gaseous form—either continuously or intermittently—and these may occur either simultaneously over large areas, or in limited streams or jets. A portion of the vapor evolved may be condensed into solid particles by the cold resulting from the rapid evaporation. In so far as the gas is in intimate physical association with the solid matter of the nucleus, it would seem that the heat of the sun would not at the perihelion distance of either Coggia's Comet, the great comet of 1861, Donati's Comet, or, indeed, of any of the conspicuous comets with a few exceptions, be intense enough to occasion such copious evolutions of gaseous matter as have been actually observed. Evolutions of occluded gas may, however, constitute the chief phenomena, produced by the solar heat, in the cases of the inconspicuous comets of short period, which, in retreating from the sun do not pass beyond the limits of the planetary system.

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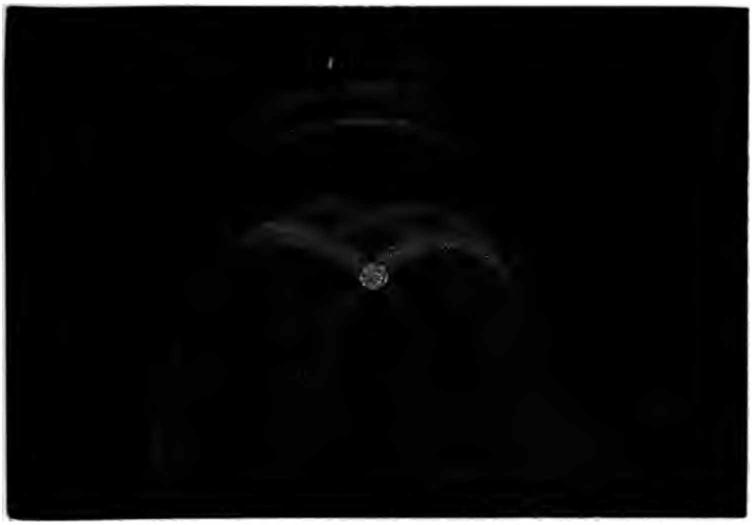
* This Journal, III, vol. ix, July, 1875, p. 44.

The conclusions that have now been reached apply strictly only to the comets that have been spectroscopically observed, but they may be regarded as probably applicable also to other comets that do not differ greatly from these in the circumstances of their approach to the sun and recess from him, and in the physical phenomena they have presented. The great comets of 1843 and 1860, that approached very near the sun, may have given off, in great abundance, aqueous or other forms of vaporous matter, derived from the liquefaction and subsequent vaporization of frozen liquids.

The important question here presents itself, what is the probable origin of the free electricity that plays so important a part in the luminous phenomena which conspicuous comets have presented? We might rest on the assumption that the solid nucleus of a comet is surrounded by a gaseous atmosphere like the earth, permeated by free electricity increasing in tension from the surface of the nucleus upward; and that whatever may be the unknown origin of the electric state of the earth's atmosphere, the same may be the origin of the atmospheric electricity of the comet. But, I will add, from my special molecular stand-point, that the ethereal electric atmospheres by which, as I conceive, the gaseous or vaporous atoms ascending from the nucleus are surrounded, should expand as these atoms recede from the nucleus, and come under diminished gaseous pressure, and so give off a certain overplus of free electricity. The outward flow of such disengaged electricity along the lines of best vaporous conduction should disengage more or less light.

Physical Aspects of the Comet.—From numerous published drawings of the comet, I select that in which its peculiar features are most conspicuous. The cut is a copy of a drawing communicated by R. S. Newall to A. C. Ranyard, showing the aspect of the comet, as seen at Ferndene on July 12th. In the accompanying description* it is stated that "the nucleus was very bright, with a disk tolerably well defined. In front of the nucleus (i. e. on the side toward the sun) was a fan-shaped light which seemed to arise from the overlapping or duplication of the two tails, which streamed away behind (the nucleus) for a length of about 15°, forming, as it were, two luminous veils, delicate, transparent and flickering, having between them a black space well defined up to the nucleus. The edges of these tails appeared to be brighter than the middle part, and crossing over the nucleus they formed the sides of the fan; the outside edges also crossing over formed the top of the fan and head of the comet. In front of this was another covering semicircular and brightest in the preceding part, and in front of that was again another fainter envelope or cloud." This outer faint envelope, or duplex envelope, has been a noticeable feature in the aspect of other comets (e. g. Donati's).



1.

Physical structure and condition of the Comet.—Upon the various drawings made by different observers, Mr. Ranyard has the following remarks:† "The drawings that were made of Coggia's comet during the early part of July, 1874, show that although there was but one small almost stellar nucleus, *there were two sets of parabolic envelopes situated side by side, and apparently overlapping one another just in front of the nucleus.* These were shown in the drawings made by Mr. Huggins and Mr. Christie. They are also to be seen in Mrs. Newall's drawing (see cut, p. 165), and they were described by Mr. Lockyer in a letter published in the Times, of July 16th, on the structure of the comet. When the comet was again visible in the Southern hemisphere, the inner duplicate structure was still visible, but the outer arcs had been dissipated." The *duplicate structure* here referred to is a highly significant fact. That it may be duly appreciated it must be borne in mind that at the period when this structure was observed, the line of sight from the earth to the nucleus was inclined under a small angle to the plane of the orbit, and under a large angle to a line through the nucleus perpendicular to the plane of the orbit. A few days later (July 21) the earth was in the plane of the orbit; the line of sight to the nucleus was in this plane, and perpendicular to the line just mentioned. We must, therefore, infer that at the period when the observations were made *the luminous jets which made up the structure observed were situated in the plane through the radius vector perpendicular to the plane of the orbit, or were seen as projected on this plane.* The lateral dispersion and lines of jet-discharge shown in the drawings, must then have been in the direction of this plane, and not in that of the plane of the orbit. Shall we then conclude that the head and first portions of the tail were flat, or approximately so, and consisted entirely of jets lying in this plane or but moderately inclined to it, or regard the drawings as showing sections of a hollow paraboloid by this plane. We must adopt the former hypothesis, since the strong contrast between the dark space behind the nucleus and the two comparatively broad bright streams on either side of it, and the sharp lines of demarcation between them cannot be reconciled with the latter.

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* Month. Notices of Astr. Soc., 1875–6, p. 279. † Ibid.

This conclusion suggests at once the inference that the nucleus of the comet must have rotated about an axis approximately at right angles to the plane of the orbit, and that the outstreaming from diverse points of the surface, had some physical relation to the latitudes of these points (i. e. their angular distance from the plane of the orbit or equator). This idea being once admitted we readily perceive that we have a plausible explanation of the duplicate structure of the comet in the hypothesis that separate streams, or systems of jets, proceeded from the two hemispheres lying on opposite sides of the plane of the orbit; and that the overlapping of the envelopes just in front of the nucleus, signalized by Mr. Ranyard, may have been the result of an initial inclination toward the radius vector of the jets issuing from each hemisphere. The prevalent notion that the outstreaming is everywhere in directions normal to the surface of the nucleus, and decreases in intensity rom the region exposed to the normal incidence of the sun's ays, it appears, then, altogether fails of application to Coggia's omet. It gives the single hollow paraboloidal tail without uplication, which is entirely at variance with the facts of obervation.

Physical Theory of cometary phenomena.—Now, if there be, n fact, two systems of jets, eminating from opposite hemisheres of the nucleus, and passing over from one to the other, re can look for the origin of such a state of things only in a upposed magnetic condition of the nucleus, and in the hypothsis that the lines of initial discharge lie in the direction of the ines of the magnetic force; or in lines having a certain relation o these. This consideration brings us to the proper point of iew for the presentation of the definite physical theory of comtary phenomena which I have been led to form, from a detailed tudy of Coggia's comet and other comets. It is briefly this: hat the direct effect of the action of the sun on the side of the ucleus exposed to the solar rays, is to form an envelope of aseous carbon dioxide extending a certain distance from the ucleus. This envelope, consisting of a diamagnetic gas, is raversed by the ideal lines of magnetic force proceeding from he nucleus, which are also lines of electric conduction through he diamagnetic gas. The electricity set free by the ascending urrents of the gas, by reason of the diminished gaseous presure, is propagated along these lines; and the impulsive force f the electric currents detaches streams of successive moleules of the gas, in the direction of the lines of conduction. e La Rive's well-known experiment of transmitting electricity hrough an attenuated gas or vapor surrounding a magnet, howed that the lines of force in the magnetic field were also ines of electric conduction, rendered luminous by the propaated electricity. The outstreaming cometic matter moves way subject to the combined action of the nucleus and sun. oth bodies exert repulsive forces upon the escaping molecules, he probable origin of which I shall consider soon. But their ffective actions may be either repulsive or attractive, according s the repulsion prevails over the attraction of gravitation, or the everse. The repulsion of the nucleus takes effect in directions ormal to its surface, or nowhere deviating much from this diection. It should be observed that besides taking effect on the aseous molecules detached by the electric discharges, it may perate on other molecules not thus detached, with an intensity ufficient to overcome their gravitation toward the nucleus.

In my mathematical discussion of Donati's comet[*] I reached he result that the tail of the comet was made up of matter of rhich a portion was solicited by an effective solar repulsion, varying in intensity from 0 to 1·218 A (A representing the intensity of the sun's attraction of gravitation at the same distance); and another portion was subject to an effective solar attraction, varying between the limits 0 and 0·455 A. I also showed that the "columnar structure" of the tail, signalized by Prof. Bond, was attributable to considerable variations, at short intervals, in the quantity of matter detached from the head of the comet; while the limits of variation of the effective solar force remained sensibly the same for several days. The varying intensity of the solar repulsion, in its operation on different molecules or particles escaping from the head, was the determining cause of their subsequent wide lateral dispersion in the plane of the orbit. Professor Bredichin, Director of the Observatory of the Moscow University, in a memoir on Coggia's comet, assigns for the effective solar force in operation on that comet, average attractive values varying from 0·118 A to 0·530 A. The values obtained in single determinations come between the limits, repulsion = 1·415 A and attraction = 0·736 A.

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[*] This Journal, II, vol. xxxii, No. 94, July, 1861.

Force of Cosmical Repulsion.—Several hypotheses have been propounded with regard to the nature and origin of this force. But none of them appear to be free from serious objections. Several years since (1861) I suggested the hypothesis that the solar repulsion might consist in the repulsive action of free statical electricity. We have abundant evidence of electric excitation both at the surface of the sun and in the cometary envelopes; but it may reasonably be doubted whether the free electricity at the sun's surface can exert, at cosmical distances, an energy as great as that which the operative cause of the wide dispersion of cometic matter displays—especially when it is considered, as suggested by Dr. W. Zenker, that a positive electric state at or near the surface of the sun implies a corresponding negative state at some lower depth.* While not prepared to admit the impossibility of a sensible electric repulsion, it appears to me that another and much more probable view may be taken of the character and origin of the solar force. This is that it is a *diamagnetic repulsion exerted by the sun as an electro-magnet* † *on the gaseous molecules of the comet.*

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* Prof. F. Zöllner, in two elaborate papers published in the Astronomische Nachrichten, No. 2057–2060 and No. 2082–2086, has endeavored to remove the force of the several objections urged by Dr. Zenker to the electric theory of the solar repulsion, and establish the adequacy of this theory. Dr. Zenker has published a detailed reply to Prof. Zöllner's arguments. As one result of the discussion, it does not appear that the above-mentioned objection has been set aside. It must be admitted, I think, that at present the electric theory rests under a cloud of doubt. As for Dr. Zenker's own reaction theory, to mention no other objections, it is certainly wholly inapplicable to the case of a comet coming as near the sun as did the comets of 1843 and 1860; and it obviously affords no explanation of the duplicate structure of Coggia's comet.

† By the term electro-magnet is meant a magnet which derives its magnetic condition from the continued operation of some external cause.

It was conclusively established by Faraday, by a series of careful experiments, that the gases, with the exception of oxygen, are diamagnetic; that is, that their molecules are repelled by both poles of a magnet. That the body of the sun is in a state of magnetic excitation there is abundant evidence. In a former publication * I have undertaken to show that the electric currents in which this must consist, may be ascribed to the impulsive action of the ether of space on the ethereal atmospheres of its molecules, resulting from the combined motion of rotation and progression. If the nucleus of a comet has a motion of rotation, as well as of revolution, the same operative cause should make it an effective magnet. It should accordingly diamagnetically repel the molecules of any diamagnetic gas or vapor that may be posited at or near its surface. The force thus exerted should increase in intensity with the augmenting magnetic intensity, up to the time of the perihelion passage, when the orbital velocity is the greatest; and subsequently decrease. The maximum of magnetic intensity may, however, be reached a certain interval of time after the perihelion passage, by reason of the persistence of currents previously developed. The magnetic condition of the nucleus would be a rapidly changing one, in approximate correspondence with the varying rate of the orbital motion. The circular magnetic currents developed would have diverse directions, in planes lying between the plane of the equator and the plane of the orbit. The effective force exerted by the nucleus on a diamagnetic gaseous molecule, would be the difference between its diamagnetic repulsion and its attraction of gravitation. The solar force of diamagnetic repulsion should vary according to the inverse square of the distance. The mathematical discussion of Donati's comet, already alluded to, served to establish that the effective solar force taking effect on the particles of the tail of the comet varied according to this law. From this fact we may infer that it probably consists of radial impulses propagated in waves through the ether of space.

Inequality of the Solar Repulsion.—Faraday established that the diamagnetism of a gas increased with its temperature. But the separate gaseous molecules of a comet being equally exposed to the sun's rays could not differ materially in temperature. Any single molecule, in receding from the sun, should, however, experience a gradual diminution of temperature, and therefore become less diamagnetic. The inequality of the solar repulsion, taking effect on different gaseous molecules, cannot then be ascribed to changes of diamagnetic condition produced by changes of temperature. But may not material variations of diamagnetic condition result from the electric discharges to which the gaseous molecules are exposed?

Looking at the matter from the general point of view I have taken in my papers on Molecular Physics, it appears that such discharges should condense the electric atmospheres (or envelopes) of the molecules, and so augment their absorptive action on the radial impulses of the diamagnetic repulsion. In proportion as the condition of the molecular atmospheres is favorable to such absorption, will the condition of the molecules be unfavorable to the reception of an impulsive action from the diamagnetic force. The tendency of electric discharges should then be to diminish the molecular susceptibility to diamagnetic repulsion from the sun or nucleus of the comet. The varying intensity of such discharges might well occasion large variations in this susceptibility. While a portion of these effects should pass off, as their orignating cause ceases to operate, a certain portion may abide as permanent changes of molecular condition. This is a legitimate deduction from my general molecular theory; from which the variability of the physical condition of the ultimate molecules under varying conditions, is one prominent inference, and the key to the satisfactory explanation of a multitude of phenomena.

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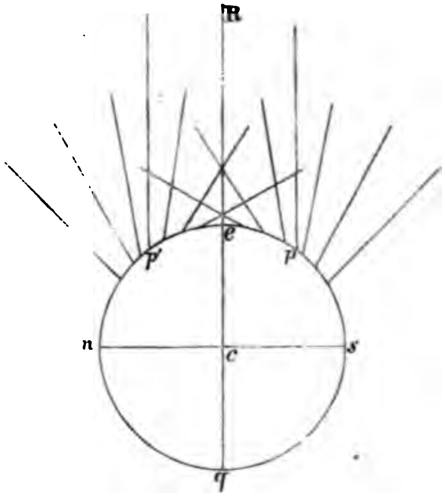
* This Journal, II, vol. xli, Jan., 1866.

The only other view that can be reasonably entertained of a possible origin of an inequality of solar repulsion, is that the cometic particles may differ greatly in size or mass. This might be allowed if we could admit that they are solid particles, or that a large number of different gases are present in the coma. But both of these suppositions seem to be irreconcilable with certain facts of observation.

Explanation of General and Special Phenomena.—It should be understood at the outset, that it is not essential to the explanation of cometary phenomena, that the force of cosmical repulsion be regarded as having a diamagnetic origin—that is, as I view the matter, consists of radial impulses propagated in ethereal waves proceeding from all points of the magnetic currents of the cosmical body. The essential feature is that a repulsion, whatever may be its origin, is exerted normally, or approximately so, to the surface of the body, and that the radial impulses of this force take effect unequally on different gaseous molecules; by reason of unequal absorption, resulting probably from certain effects produced by electric discharges—but possibly on solid cometic particles differing in size or mass. A magnetic condition of the nucleus, sufficiently decided to determine lines of electric conduction through its diamagnetic gaseous envelope, coincident with the lines of force in the magnetic field, is however an essential feature of the theory.

The precise character of the phenomena should depend, to some extent, on the position of the axis of rotation of the nucleus. We may regard as the normal position that in which it is perpendicular to the plane of the orbit: and the attendant phenomena as the general phenomena. If the axis deviates from this position there will be two efficient causes of special phenomena to be considered: (1) the point of maximum evaprating effect of the sun's rays will probably fall at some point of either the northern or the southern hemisphere of the nucleus, nstead of on the equator; (2) the magnetic poles will not coincide with the poles of rotation, and the magnetic equator will be more or less inclined to the plane of the equator of rotation, as well as to that of the orbit. From this it follows that the lines of force will, in general, be more or less inclined to the astronomical meridian planes of the nucleus; and hence that the initial directions of the jets of cometic matter will be inclined to these planes.

Let us first take the axis in its normal position. We at once perceive that there may be two general varieties in the process of evaporation under the influence of the sun's rays. (1.) A continuous evaporation most abundant at the equator, and extending into both hemispheres. (2.) A more copious evaporation occurring only at certain intervals, beginning in each instance at the equator, and subsequently extending both north and south. A tendency to such intermittence at each point of a meridian should result from the cold produced by a copious evaporation and the intercepting action of the vapors already generated. The vapor or gas developed by either of these special processes becomes subject to an expulsion to an indefinite distance, either by an effective repulsion exerted by the nucleus, or by the impulsive force of electric discharges combined with the effective force exerted by the nucleus, whether attractive or repulsive. In both of these modes of expulsion the gaseous molecules, in their motion *relative to the nucleus*, are subject to the full force of the sun's repulsion, undiminished by his attraction of gravitation. Those which experience the first mode of expulsion, not having suffered any diminution of susceptibility to diamagnetic repulsion by reason of electric discharges (p. 170), are energetically repelled both by the nucleus and sun. They become separately visible in the straight "secondary tails" (e. g. Donati's comet). On the other hand, the gaseous matter expelled by electric discharges, serves to form the envelopes that rise in succession from the nucleus (e. g. Donati's comet; great comet of 1861). In the formation and expulsion of each successive envelope, whichever of the two processes of evaporation may occur, the evolution of gaseous matter will be most abundant at the equator, and electric discharges may reasonably be supposed to begin near the equator, and extend gradually both north and south. The varying directions of the discharges, and therefore also of the initial directions of the jets, from diverse points of a single meridian, are shown in Fig. 2. At about 35° from the equator, on either side, the direction becomes parallel to the equator (*ecq*), and thus to the radius vector (*ceR*). From 0° to 35° it tends toward this line, and beyond 35° diverges from it under a larger and larger angle. If the initial velocity is constant, as well as the solar repulsion, the escaping molecule should attain to its greatest distance from the nucleus when the discharge occurs from the latitude of 35°, and in the precise direction of the sun. If then the process of electric discharge begin near the equator, and extend gradually to the north and south, the outer surface of the envelope formed will gradually move away from the nucleus, and attain its greatest distance when the process reaches the latitude 35°. The jets that issue from latitudes greater than that (about 35°) at which the direction is parallel to the radius vector, do not pass sensibly beyond the boundary line of the jets that proceed from points between 0° and 35° of the other hemisphere, unless for such jets the projectile velocity is greater, or the solar repulsion less.*



2.

What has now been stated should be the precise result if the molecules after receiving a projectile velocity were exposed only to the retarding force of the solar repulsion. The effective action of the nucleus would obviously modify somewhat the curve for each initial direction, and alter the limiting latitude for which the recess in the assumed direction is a maximum. If this effective action is attractive, this latitude will exceed 35°, if it is repulsive, it will be less than 35°.

We may infer from the great dimensions of the head of a comet in comparison with the nucleus, that the action of the nucleus becomes practically insensible at a small fraction of the distance to which the jets recede in the direction of the sun. We may then regard any expelled particle as issuing from the small sphere of sensible action of the nucleus with an initial velocity resulting from the projectile force of the electric discharge, and the retarding force of the nucleus (or accelerating force if the effective action should be repulsive); and as subsequently retarded by the solar repulsion. As we have seen, p. 170, the gaseous molecule which receives the greatest electric impulse should thereafter be the least susceptible to repulsion by the sun or nucleus. Such a molecule, though receiving the highest projectile velocity, should then be most retarded by the effective attraction of the nucleus (or least accelerated by its effective repulsion). The tendency of this state of things is then, to bring the velocities of all the different molecules, as they emerge from the sphere of sensible action of the nucleus, to approximately the same value. In considering the subsequent motion of a molecule, under the retarding influence of the solar repulsion, its velocity and direction of emergence from this sphere, becomes the *initial* velocity and direction.

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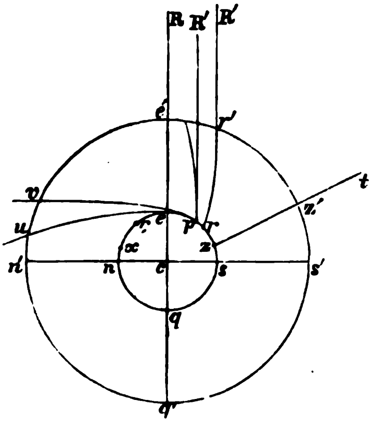
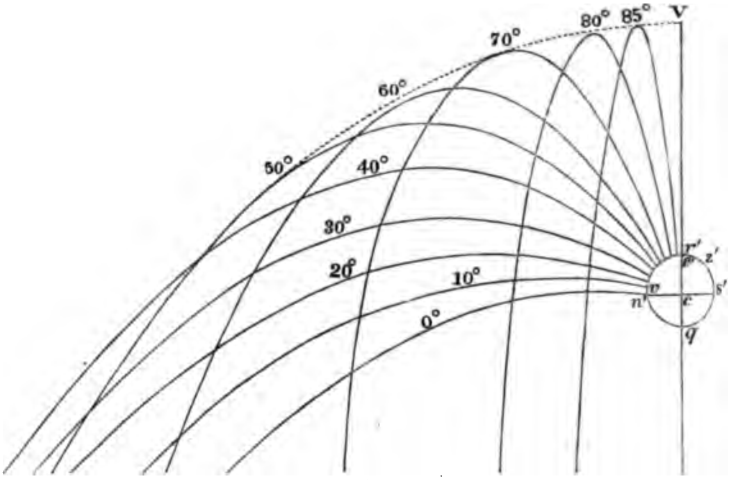
* See remark about recess of envelopes on p. 176.



3.

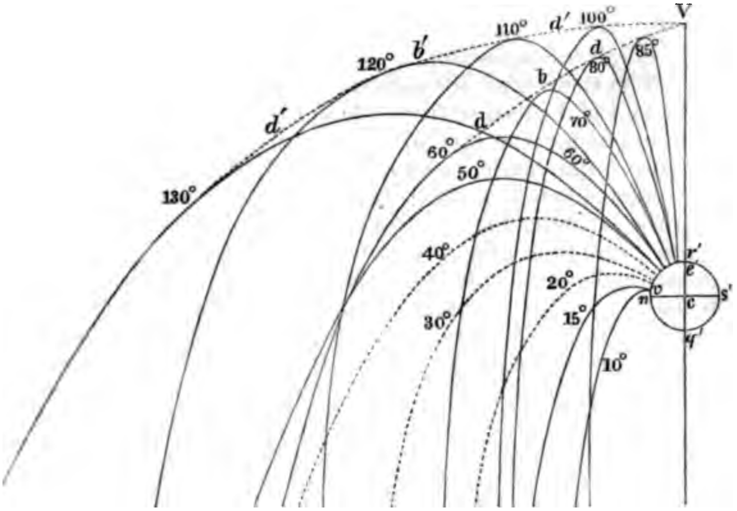
As, for any one molecule, the solar repulsion is sensibly constant within the extent of the head of the comet, the path of the molecule will be parabolic. If we regard the initial velocity as constant for the different molecules, those which suffer the least retardation from the solar repulsion should attain to the greatest distance from the nucleus; and at the time of greatest recess lie in the outer surface of the envelope. Those which suffer the greatest retardation should form a similar luminous surface at a lower depth. The latter would eventually form the preceding side of the tail of the comet, from a certain distance behind the nucleus, while the former would form the following side. The time required for any receding particle receiving an initial velocity in the line of direction of the sun, to reach its point of greatest recess, must have been less than twenty-four hours, in the case of the two comets (Donati's and Coggia's) for which the intensity of the actual solar repulsion has been determined.

Let $n\,e\,s\,q$ (Fig. 3) represent the nucleus, or more strictly the surface of projectile discharge, and $n'\,e'\,s'\,q'$ the sphere of sensible action of the nucleus; $e\,q$ being the equator and $c\,e\,R$ the direction of the sun. The point p is at lat. 35°, and r is the point of projection of the jet which issues from the sphere $n'\,e'\,s'\,q'$ in the direction $r'\,R'$ parallel to $c\,e\,R$. The jets proceeding from the arc $e\,r$ issue from this sphere at various points from u to r'. One of these jets proceeding from a certain point near e will have at v a direction at right angles to $c\,e\,R$.



4.

If the jet discharge extend beyond r to some point z the jet proceeding from z will emerge from the sphere $n'\,e'\,s'\,q'$ at z', in a direction $z'\,t$, having a certain inclination to $c\,e\,R$. The cometic matter outstreaming from the arc $r\,z$ will emerge from the same sphere between r' and z'. Now let $n'\,e'\,s'\,q'$ (Fig. 4) answer to $n'\,e'\,s'\,q'$ in Fig. 3, and let α denote the inclination of any jet emerging from this sphere to the line $n'\,c\,s'$ perpendicular to the radius vector $c\,e'\,R$. Suppose α (estimated from left to right) to increase by 10° from 0° to 90°, and let us regard the initial velocity, on leaving the sphere $n'\,e'\,s'\,q'$, as constant, (p. 173) and also the solar repulsion as constant for each jet. The parabolic curves that would be pursued are shown in the diagram. The jets proceed from *n′ v e′ r′*, and were projected from the lower latitudes of the right hemisphere, or between *e* and *r* (Fig. 3). Jets issuing from the corresponding arc *e* $r_{,}$ of the other hemisphere, would form a similar set of curves to the right of *c e′ R*. Other sets of parabolic jets are projected from latitudes above 35°, or from the arcs *r z* and $r_{,}$ *x* (Fig. 3); but these will have sensibly the same outer boundary as those from *e r′* and *e*$r_{,}$ unless the initial velocity or the solar repulsion is different. It is to be observed that *c e* is so small a fraction of *c e′* (Fig. 3) and *c e′* so small a fraction of *c* V (Fig. 4) that we may without material error regard *v* as coincident with *n′*, and *r′* as coincident with *e′*.



5.

The hypothesis made in the construction of Fig. 4, that the solar repulsion is constant for all values of *a*, or in other words that the force of electric discharge on which the intensity of this repulsion theoretically depends, is independent of the initial angle of direction of the jet, is not the most probable one under the circumstances. In fact, the electric discharges at the nucleus should apparently increase in intensity and impulsive action, with the inclination of the line of discharge to the surface, and hence the solar repulsion should, according to the theoretical principle before stated (p. 170) diminish as this angle of inclination increases. This angle corresponds to the dip of a magnetic needle on the earth's surface, and varies, we may suppose, with the latitude according to a similar law. Let it be denoted by β.

In Fig. 5 the solar repulsion is assumed to be inversely proportional to sin β. The change of direction of the jet while passing through the sphere of sensible action of the nucleus is neglected in the application of this law. Two systems of curvilinear jets are shown—one answering to values of a varying by 10° from 50° to 90°, and emanating from latitudes less than 35° of the right hemisphere, or from a portion of the arc $e\,r$ (Fig. 3); and another answering to values of a varying by 10° from 90° to 180°, and estimated from right to left. These emanated from latitudes greater than 35° of the left hemisphere, or from a portion of the arc $r_{,}\,x$ (Fig. 3). The other corresponding systems of jets, emanating from portions of $e\,r_{,}$ and $r\,z$, would form the other half of the comet. If we consider all the jets issuing from the right hemisphere as forming one system, and all those issuing from the left hemisphere as forming another system, then for each system a will vary from 50° to 180°: and will increase from left to right for the first, and from right to left for the second. They answer to the supposition that the outstreaming extends from latitude 17½° to 57½°, in each hemisphere. It will be seen that the jets proceeding from the latitudes higher than 35°, for which a varies from 90° to 180°, recede to greater distances from the nucleus than those emanating from latitudes less than 35°, for which a varies from 50° to 90°. This of course results from the greater values of sin β that obtain for the first-mentioned set of jets. Thus, for the 130° jet sin $\beta = 0{\cdot}953$, while for the 50° jet sin $\beta = 0{\cdot}534$. The envelope, $V a b d$, or outer boundary of the latter system of jets thus falls within that, $V a' b' d'$, of the other system. The overlapping of the two systems of jets should accordingly be conspicuously visible, and an appearance presented similar to that of Coggia's comet on July 12th, as shown in the drawing on page 165.

This drawing indicates that the evolution of cometic matter was much less for angles a from 0° to about 50° than from 50° to 130°. Not to confuse the drawing too much I have shown in full lines only two jets between 0° and 50°, answering respectively to 10° and 15°; as showing that the boundaries of the dark space behind the nucleus were probably formed by jets issuing from low latitudes on the nucleus.

It ought to be added, as an important feature of the general theory, that the recess of a cometary envelope, or system of jets from the nucleus, should continue after the jet discharge has reached the latitude 35° and upward, until the electric discharges at these latitudes have attained to the maximum intensity.

Certain comets have presented peculiarities of appearance which I find, on a careful examination, admit of satisfactory explanation on the hypothesis that the equator of the nucleus was inclined to

the plane of the orbit, and in consequence the two magnetic hemispheres were unequally exposed to the influence of the sun's rays. The special features alluded to, are the unsymmetrical position of the envelopes thrown off in succession from the nucleus (e. g., the envelopes of Donati's comet in their earlier stages, and especially the five separate envelopes of the great comet of 1861, as seen by Dr. Schmidt on June 30th); and great differences in the length of the two branches of the normal tail, in connection with an anomalous curvature of the first portion of the longer branch, when the comet was viewed from certain positions of the earth relative to the plane of the orbit, observed in the case of Comet II, 1862, and elaborately discussed by Prof. Schiaparelli and Prof. Bredichin. In this case we have only to suppose that the sun was vertical to points of one of the hemispheres, and as a consequence the jet discharges were mostly confined to that hemisphere. The longer branch of the tail was composed of jets issuing from the lower latitudes, while the less copious and more fluctuating discharges of matter subject to a diminished solar repulsion (p. 176) from the higher latitudes, formed the shorter branch of variable length.* The anomalous curvature of the former system of jets, and their interlacing with the other system, was a simple consequence of the greater intensity of the solar repulsion in operation on the former than on the latter.

The curious phenomenon of the *oscillation of jets* first observed by Bessel in the head of Halley's comet, and of which he offered in explanation the improbable hypothesis of a polar attractive force exercised by the sun upon the nearer portion of the nucleus, may be seen to be another probable consequence of an inclination of the equator to the plane of the orbit. It has already been intimated (p. 171), as one theoretical result of such a state of things, that there should be two magnetic poles in each hemisphere, and that the lines of magnetic force should be variously inclined to the planes of different local meridians on the nucleus. As the planes of these different local meridians are brought successively by the rotation into coincidence with the meridian plane through the sun, the lines of jet discharge, which are coincident with the lines of magnetic force, should oscillate with respect to this plane.

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* To illustrate, if the outstreaming were confined to the right hemisphere, (Fig. 3) the jets proceeding from *e r* would pass over to the left side of the nucleus, and those proceeding from *r s* would curve around to the other side.

ART. XXII.—*On the Velocity of Transmission of Earth Waves;* by General H. L. ABBOT, Corps of Engineers.

ADVANTAGE was taken of the explosion of 50,000 pounds of dynamite at Hallet's Point, on September 24th, 1876, to measure the velocity with which the shock was transmitted through the ground, both across Long Island and along the south bank of East River. The results were embodied in a paper read by me before the National Academy of Sciences, on October 18th, 1876, and subsequently again read and printed as one of the papers of the Essayons Club of the Corps of Engineers.

In the number of the London, Edinburgh and Dublin Philosophical Magazine, for October, 1877, appeared a short review of this paper from the pen of Mr. Robert Mallet, F.R.S., a gentleman well known for his numerous and able contributions to seismology. In this article, he suggested reasons which led him to doubt the value and accuracy of the Hallet's Point results.

Even if I had felt disposed to enter into a controversy upon the subject, I should have been quite disarmed by the concluding sentence of this article, which expresses views so just and liberal that it may well be quoted as an exemplar of the manner in which scientific questions should be considered. He writes:

"In these objections I wish to be clearly understood as having no *a priori* difficulty in accepting a higher velocity of wave transit than the highest attained experimentally by myself. It is highly probable that such may be elicited by future experiment. But should such cases arise, their results like all great physical truths, should only be credited upon unexceptionable observations or experimental evidence. While feeling justified in making these objections, I wish to disclaim all controversial spirit or intention; loss of sight, indeed, and diminished energy would prevent my engaging in any scientific controversy, were any called for."

Believing, at the date of my first paper, that the data secured at the Hallet's Point explosion demonstrated the necessity for more exact and comprehensive knowledge of the subject, I have, during the past season, taken advantage of the facilities offered by large sub-aqueous explosions at the School of Submarine Mining at Willet's Point, to continue the investigation; and, on October 23d, 1877, I read a second paper before the National Academy of Sciences, giving the results thus obtained. As only a brief abstract of this paper has appeared in print, I propose now to give a summary of the conclusions suggested by the whole series of experiments and, incidentally, to explain my reasons for believing that Mr. Mallet has not quite understood the parts of my first paper to which he has taken exception.

Limited space forbids any detailed explanation here of the method adopted for measuring the time of transmission of the shocks; especially as this is fully given in my printed paper, together with the notes of the observers in full. Suffice it to say that the instant of explosion and the time of arrival of the tremors, were electrically recorded on the same moving paper with extreme precision. The following table exhibits the data, of which only the first six observations were known to Mr. Mallet when he wrote his article.

No. of observations.	Date.	Observer.	Cause of shock.	Distance to station.	Type of seismometer.	Tremor of mercury.		Velocity of transmission.
						Arrived in	Lasted for	
				miles.		secs.	secs.	ft. per sec.
1	Aug. 18, '76.	Capt. Livermore	200 lbs. dynm.	5 ±	B.	5 ±		5280 ±
2	Sept. 24, '76.	Lieut. Young	Hallet's Pt. ex.	5·134	A.	7 ±	63 ±	3873 ±
3	" " "	Lieut. Griffin	" " "	8·330	B.	5·3	72·3	8300
4	" " "	Lieut. Kingman	" " "	9·333	A.	10·9	23·5	4521
5	" " "	Lieut. Leach	" " "	12·769	B.	12·7	19·0	5309
6	Oct. 10, '76.	Lieut. Kingman	70 lbs. powder.	1·360	A.	5·8	inst.	1240
7	Sept. 6, '77.	Lieut. Kingman	400 lbs. dynm.	1·169	A.	1·8	7·8	3428
8	" " "	Lieut. Leach	" " "	1·169	B.	0·7	17·8	8814
9	Sept. 12, '77.	Lieut. Griffin	200 lbs. dynm.	1·340	A.	1·05	8·8	6730
10	" " "	Lieut. Leach	" " "	1·340	B.	0·81	17·1	8730
11	" " "	Lieut. Griffin	70 lbs. powder.	1·340	A.	1·27	4·8	5559
12	" " "	Lieut. Leach	" " "	1·340	B.	0·84	15·1	8415

Mr. Mallet's results were reported many years ago to the Royal Society, and unquestionably are to be accepted as exhibiting the velocities due to his initial shocks and given by the method which he employed. Unfortunately, no full description of this method, and especially no record of the optical power of the instrument, is contained in his paper to the Royal Society; and my results, as will soon appear, prove the latter to be of unexpected importance. The following are his figures:

Velocity in ft. per second in sand .... 825 ft.
" " " in discontinuous and much shattered granite ....1306 ft.
" " " in more solid granite ....1665 ft.
" " " in quarries at Holyhead (mean) 1320 ft.

The extraordinary differences between these rates and those measured at the Hallet's Point explosion, and the apparent discrepancies of the latter among themselves, led me to so plan the new observations as to throw light upon two points: 1st, Does a telescope of high power detect a tremor in the mercury in advance of the one first revealed by a lower power? 2d, Is there a difference in the rate of transmission of the shocks, due to differences in the intensity of the initial explosion?

The method adopted was to station two observers near each other at a carefully selected inland position; each observing a mercury seismometer, and holding in his hand the key of an accurate Morse register to record the instant of arrival and the duration of the tremor. These seismometers were the same instruments used at the Hallet's Point explosion. They differed from each other only in the optical power of the telescope, that designated A in the table having a magnifying power of 6, and that marked B of 12.

A fuse in the electrical circuit which fired the torpedo, was bedded in the cartridge of a field gun directed toward the observers; and, as a very powerful battery was used, the two explosions were absolutely simultaneous. A third observer, holding in his hand a key which closed the circuit of both registers, observed the muzzle of this gun with a good telescope, and recorded the flash electrically with precision. The observers were all officers of the Corps of Engineers accustomed to delicate observations. The distance traversed by the shock was measured by triangulation.

The observations of September 6th and September 12th, 1877, marked 7 to 12, and sufficiently explained in the foregoing table, answered the query relative to optical power decisively in the affirmative; and, as may be seen from the table, the earlier observations singly and collectively suggest the same conclusion. The mean velocity given by the six observations with type A is 4225 feet per second, while that given by the same number with type B is 7475 feet per second. There can, therefore, be no doubt that the first tremor of the mercury is of too feeble intensity to be detected with a magnifying power of 6. Hence, as the first tremor determines the true rate of transmission of the shock, the rejection of all the observations made with type A follows as a matter of course. They are valuable as exhibiting the rate of advance of waves having a certain intensity, but they do not reveal the velocity of the leading tremor. Indeed it is not impossible that a power above 12 might have detected a still earlier tremor; or, in other words, have indicated velocities of transmission exceeding those in the table.

These conclusions are so novel and important that certain additional evidence will be given, furnished by the notes of the observers recorded at the time and without consultation.

Lieut. Leach, using a power of 12, recorded of the second observation of September 12th (No. 12), "The gunpowder wave was peculiar, in having a much more gradual increase than has been observed in dynamite shocks. I should say it was at least two seconds in attaining a maximum, whereas the dynamite usually reaches its maximum in a very small fraction of a second."

Lieut. Griffin, who used a power of 6, recorded of the same shock (No. 11), "The gunpowder disturbance remained constant for a perceptible interval, instead of immediately decreasing as is the case with dynamite."

It will be noticed that the shock of October 10th, 1876 (No. 6), was caused by the explosion of seventy pounds of gunpowder—the same charge which was fired on September 12th, 1877, and to which these records refer—but that the duration of the tremor was much less. The reason is, that the initial earth waves were far more violent in the latter case, when the torpedo lay on the bottom in thirty feet of water, than in the former, when the charge was only submerged five feet in water thirteen feet deep, and thus expended much of its energy in throwing a huge jet of water 330 feet into the air.

Thus it will be seen that these records, and the velocities observed on the two days, all tend to confirm the idea that a slow-burning explosive, like gunpowder, generates a series of gradually increasing tremors which, at a distance of a mile, are at first quite invisible with the less sensitive seismometer; and are only detected by it when near their maximum intensity. If Lieut. Leach's estimate of time for the arrival of the maximum wave be accepted, we have, therefore, for the first mile:

Deep torpedo (70 lbs.)	Power of 12 gives	8415	feet per second.
	" " 6 "	5559	" " "
	Estimated minimum power gives	2489	" " "
Shallow torpedo (70 lbs.)	actual minimum power gives	1240	" " "

Without wishing to attach undue weight to these records, they appear to me to suggest a possible explanation of the discrepancies between the results obtained by Mr. Mallet and myself, provided his magnifying power was low. It would be very interesting to know the actual power employed.

As to the second query—Whether the rate varies with the intensity of the initial shock, the data, although less decisive, certainly suggest an affirmative answer. The evidence afforded by No. 6 and No. 11 has been already pointed out. For a power of 12, the table shows that for the first mile:

400 lbs. of dynamite give 8814 feet per second.
200 " " " " 8730 " " "
70 " " powder (deep) 8415 " " "

If it be admitted also that the velocity of the wave diminishes with its advance, all the data become accordant. Thus:

200 lbs. of dynamite give for 1 mile, 8730 ft. per second.
" " " " " " 5 " 5280 " " "
50,000 " " " " " 8 " 8300 " " "
" " " " " " 13½ " 5300 " " "

In fine, I believe that the following general conclusions are suggested by these measurements: 1st, A high magnifying power of telescope is essential in seismometric observations. 2d, The more violent the initial shock the higher is the velocity of transmission. 3d, This velocity diminishes as the general wave advances. 4th, The movements of the earth's crust are complex, consisting of many short waves first increasing and then decreasing in amplitude; and, with a detonating explosive, the interval between the first wave and the maximum wave, at any station, is shorter than with a slow-burning explosive.

A summary of the objections suggested by Mr. Mallet to the Willet's Point observations will now be given; although, in the new light thrown upon the subject by these subsequent measurements, perhaps he might not desire to press them.

Mr. Mallet considers that the explosion at Hallet's Point furnished data ill suited to determine the delicate physical problem of the rate of progression of a wave of disturbance through the earth's crust, for the reason that the initial impulse was due to the combined effect of many small charges, and not of one single mass of dynamite; hence, as he states:

"I conceive it, therefore, impossible with sufficient exactitude to assign the instant at which the wave of shock started in this instance; and to assume the appearance of disturbed water above the seat of explosion, or the shock felt from the ground closely adjacent to it, as marking that instant seems to me, in either choice, to neglect many sources of error."

Being unable to read my paper personally, he has not fully understood the manner in which the instant of the starting of the wave was fixed. The great mine was fired by a mechanical drop, which closed simultaneously all the twenty-three electrical circuits. One extra pin and mercury cup in this drop was provided by General Newton for my use, and the signal was thus automatically sent over the telegraph line to the several stations. Human judgment had no part to perform. The absolute instant of closing the battery circuits was electrically recorded on all the register papers. It is impossible that this could have *followed* the explosion; and if it preceded it, by a differential of time, the effect upon the records would be to indicate *too slow* and not *too fast* a rate, and thus to bring my results more nearly into accordance with those of Mr. Mallet.

Mr. Mallet next argues that the discrepancies of the four records among themselves invalidate their accuracy. I have just shown how these apparent discrepancies are explained by the later observations at Willet's Point; and have thus answered this argument which, when the results were first collated, suggested itself to my own mind.

Mr. Mallet next mentions certain objections that show him not quite to understand the geography and geology of the region separating Hallet's Point from the four stations; which, in the absence of a map to illustrate the paper, is very natural. The straight line to Willet's Point follows the shore and waters of East River, offering both a land and water route to the wave of disturbance. The other three stations lie in the interior of Long Island, with no water between them and Hallet's Point. The intermediate country consists of low rolling hills formed of deposits of the clay, sand and boulders characteristic of drift, the geological formation to which they belong. The stations were carefully selected to detect, if possible, what influence is exerted upon the rate by intermediate sheets of water; and the fact that the wave reached Willet's Point with a higher velocity than the inland stations is, perhaps, confirmatory of the idea that water is a better conductor than land.

Mr. Mallet refers, as causes of uncertainty, to three sets of waves generated: (1) by the explosion, (2) by the fall of the rock, (3) by the fall of the water; and also to the wave-diffusion experienced in traversing long distances. He argues, therefrom, that the distances were too great for satisfactory observations. Now the records show both the beginning and end of the mercury vibration, as well as the automatic signal sent by the explosion. The first and last determine the velocity of the advanced wave, which is that given in the table; the second and last approximately fix the time of arrival of the last wave, and enable its velocity to be roughly estimated. The physical phenomena following so exceptionally large an initial shock are thus made a matter of record. As to the element of distance, it seems to me that the reduced probable error in the measurement of the time of transit, should give the preference to long over short routes, when the velocity is so high as that under consideration.

As to the precision with which these distances were determined, Mr. Mallet expresses doubt; but since they depend upon the exact triangulation of the Coast Survey, supplemented by careful railroad surveys, no scientific man who is familiar with the geodesy of this region will hesitate for an instant to give them full credence. The probable error due to length of route, as compared with that due to any possible measurement of so short a time, is practically zero.

Mr. Mallet suggests that a measurement of the personal equation of the observers should have been made. This was not done, because absolute, not differential times were to be observed; and no absolute personal equation machine was available. The officers were trained observers; and their distances from Hallet's Point were so considerable, that a small personal equation would probably exert no material influence on the relative velocity over the different routes.

In fine, my own opinion is that in this, as in all physical investigations, the more thoroughly the ground is covered the greater will be the number of important facts developed; and that complex rather than simple phenomena should be expected. It is quite impossible to ascribe the discrepancy between Mr. Mallet's results and those here described to inaccuracy of observation. Differences in the material traversed by the waves, and in the method of observing, may possibly explain them. If Mr. Mallet's health will permit him to publish the details of his mode of observing, and to give the whole subject a general discussion, the paper will certainly find interested readers.

Willet's Point, N. Y. Harbor, Jan. 14, 1878.

ART. XXIII.—*On Systems of Chemical Notation.* Letter of M. BERTHELOT to M. Marignac* (from the Moniteur Scientifique of December, 1877).

ALLOW me, in the first place, to correct the opinions concerning the teaching of chemistry in France, which I find expressed in your article, and which have been propagated, perhaps intentionally, in foreign countries. There is no regulation which makes it obligatory on professors of the Faculties to adopt any particular notation, and, as a matter of fact, both notations, by equivalents and by atoms, are about equally represented in our lectures. At the Sorbonne, MM. Würtz and Friedel have adopted the system of atoms; the College of France presented last year a professor who uses the atomic notation; I was commissioned to make the report to the Minister, who gave his approval and made the nomination. In examinations both notations are equally accepted, and, if any pressure exists on the candidates, it is exerted by the partisans of atoms rather than by the others. Officially, therefore, perfect freedom exists on this question. If the atomic notation has not been generally accepted in France, it is because it has not succeeded, so far, in obtaining the good opinion of the majority of scientists; but, notwithstanding, imputations have not been spared that the partisans of equivalents are animated with a retrograde spirit.

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* An answer to the paper in Moniteur Scientifique, September, 1877. (See this Journal, February, 1878, p. 89.

Allow me, in the next place, to point out some observations on the fundamental part of the question under discussion. It presented itself before the Paris Academy of Sciences under two heads: the system of atoms, and the language or notation of atomic weights. You were right in separating these two things. I had tried to do the same thing, but with less distinctness, in my last work, *On Chemical Synthesis*, in which I explained the system very fully, but without adopting it, and I said that the notation by atoms possesses certain advantages, but also some disadvantages. The discussion recently raised could not, in the nature of things, assume this methodical form; but I believe that I kept about the same ground, as I always said that the two languages expressed the same ideas in the same way, in most cases, except that special advantages belonged to each system of notation. Your conclusions seem to be about the same as mine.

The definition of equivalents, which you accuse me of not giving, was nevertheless presented during the discussion, and I will take the liberty of reproducing it: "Equivalents express, in my opinion, the ratios of weight according to which bodies combine or substitute themselves for one another." These ratios may be determined by the balance with infinitely greater precision than can be ascribed to most physical laws. As, however, experience proves that bodies combine according to several proportions, which are multiples of one another, it follows that equivalents themselves are only determined within an approximation of a multiple of a certain unity, precisely as axes are determined in crystallography. The choice of the unity belonging to each body is therefore somewhat arbitrary. It may be determined from purely chemical considerations, which are never wanting, by taking the weight which agrees the best with the general reactions of the body, which affords the simplest form, and that which conforms the best with analogies.

These analogies are generally expressed by precise rules which are founded on the reciprocal substitution of metals and metalloids, the formation of oxides and acids, and their reciprocal combinations, and the multiple proportions according to which elements combine. In only one case, that of alumina, have we had to appeal to more delicate analogies, drawn from the existence of a remarkable class of double salts, which have been corroborated by the general resemblance of the salts of this base with those of the sesquioxides. It is only in a subordinate way, and for the purpose of giving greater precision to chemical analogies, which are often somewhat vague, that physical properties have been introduced, such as the gaseous density, the specific heat, the crystalline form, the molecular volume in the solid state, etc.

The part which physical properties are to play in the determination of equivalents, and the relative importance of these properties seem to me to constitute the only difference between your views and mine. If it was possible to make equivalents agree exactly with gaseous densities, as the old atomic school had hoped to accomplish, the numbers obtained would probably be adopted by all chemists. This has happened in organic chemistry, in which the same equivalent weights are accepted by every body. Unfortunately, however, this concordance does not exist in mineral chemistry, whence the attempt of the new school to establish atomic weights by means of specific heats. But I persist in the opinion, although I am sorry to find that it is opposed to yours, that this base has not sufficient theoretical solidity when it is in contradiction with the gaseous densities. The specific heats of simple gases, which obey the laws of Mariotte and of Gay Lussac (an obedience which is expressed by the constancy of their gaseous densities), are necessarily the same under the same volume, because the specific heat measures the work accomplished in fulfilling these laws. If the specific heats of the elements in the solid state do not observe the same ratios as in the gaseous state, it is for one of the two following reasons, either the specific heats of the solid elements change unequally with the temperature, as I believe is the case, or two gaseous molecules are united in one solid molecule, as the atomists suppose. In either case, it seems to me that the specific heats of solids must be put aside in the determination of absolute equivalents.*

I insist the more on this point that the new equivalents, if we attribute to this word the extensive meaning that you rightly give to it, introduce an undeniable complication in chemical reactions. In your classical researches on the specific heats of saline solution you found yourself obliged to double the atomic weights of hydrochloric and of nitric acid and of their salts, with the object of expressing with greater clearness the analogies and parallelism of their properties. You wrote:

$$H^2Cl^2;\ Na^2Cl^2;\ N^2O^5,\ H^2O;\ N^2O^5,\ K^2O,$$

and in the same manner you were led to double acetic acid and the acetates:

$$C^4H^6O^3,\ H^2O;\ C^4H^6O^3,\ K^2O.$$

The same necessity has been felt by all those who have had to express the equivalent ratios of acids, of water and of bases, as may be seen in the remarkable papers of Mr. Thompson on Thermo-chemistry, and even in the new edition of Gmelin, now publishing in Germany (see, among other things, iodic acid I^2O^5, H^2O).

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* I cannot accept your opinion on the absolute value of the law of Wœstyn in the calculation of the specific heats of solid compounds. You know very well that M. Kopp, who went to the bottom of this question in 1864, found himself obliged, in verifying this relation, to attribute to the solid elements in their combination specific heats varying from 6·4 (silver, chlorine, nitrogen), down to 4 (oxygen), 2·3 (hydrogen), and 1·8 (carbon).

The agreement of the numbers adopted by the partisans of atomic weights is then more apparent than real.

But I have no wish to prolong this controversy, particularly, as, between us, the only question is as to ascribing one value or another to the unity, of which the various equivalents of the same body are multiples. If we confine the question within these limits, it certainly does not present the excessive importance which has been ascribed to it for the last twenty years. The new atomic school has not, it appears to me, justified its pretension of changing the very base of chemical doctrines, and of founding a *new chemistry*, essentially different from the old. The only thing it has done has been to intermix the meshes of its hypotheses with our demonstrated laws, and this to the great detriment of the teaching of positive science. I believe that it would be advisable, in the future, to set aside all these systems, and to turn the minds of young scientists towards the really new views offered by molecular mechanics, which promise such rich harvests of discoveries.

Benzeval-sur-Dives (Calvados), August 10th, 1877.

ANSWER OF M. MARIGNAC.

I cannot but esteem myself very happy that the remarks which I recently presented in the *Moniteur Scientifique* on systems of chemical notations have given rise to the interesting article which precedes. I would like, however, to add a few remarks.

I found this fault with chemical equivalents, that they are not susceptible of a precise and general definition. M. Berthelot, while opposing this doctrine, seems to me to confirm it, as he was obliged to give a double definition. Sometimes these equivalents represent the ratios according to which bodies substitute themselves to one another; this is certainly a precise conception, which I have called *true chemical equivalence*, but it can only be applied to restricted cases. At other times, they are the ratios according to which bodies combine, or rather one of the ratios, which are multiples of one another, according to which combinations take place; in this case, the question of equivalence remains undetermined and more or less arbitrary. Besides, we may add that it is not always easy to see why one or the other of these definitions can be applied to the equivalent of a body. For instance, aluminium, compared to the metals which are most nearly related to it, those of the earths and alkaline earths, has a perfectly determinate value of substitution, and still this is not the value that has been chosen for its equivalent.

As M. Berthelot himself observes, we only differ in the opinion that each of us has formed on the part and relative importance which are to be ascribed to physical properties in the determination of equivalents. I may possibly have an exaggerated idea of the importance of these properties. But does not M. Berthelot, on his side, labor under a delusion when he thinks that chemical considerations alone are a sufficient guide to a chemist in this determination; is he not under the influence, which he has often discovered in his opponents, of a state of mind in which things seem very natural because we are accustomed to accept them? If the equivalent adopted for aluminium was not confirmed by the specific heat of this metal, by the vapor density of its chloride, and by numerous considerations of isomorphism, is he very sure that we would not hesitate between the formulas Al^2O^3 and AlO for alumina? What would make us suppose that hesitation would be permissible is the great number of bodies in which the determination of the equivalent has remained doubtful as long as physical properties have not served as guides, such, for instance, as in silicon, zirconium, glucinum, and the numerous group of the metals of cerite and gadolinite. The only thing I ask is to allow the same importance to physical properties in the case of bodies which occur with frequency.

M. Berthelot finds fault with me for being in apparent contradiction with myself, because in my researches on the specific heats of saline solutions, I doubled the molecular weights of some compounds, to better express the parallelism of the properties of some compound groups. It is true that, in comparing with one another the salts of the same acid, it has seemed to me more natural to refer their properties to equivalent quantities, or to quantities containing always the same proportion of acid. But if this has led me to group together two molecules of an alkaline chloride or nitrate, I was obliged, for the same reason, when taking the specific heats of sulphate of alumina or of alkaline phosphates to take, as unities of the weights of these salts, quantities which are really equivalent of other bodies, but which only represent fractions of the admitted equivalents of these bodies. Notwithstanding, M. Berthelot does not conclude that this is a proof that alumina should be written AlO and phosphoric acid $PhO_{\frac{5}{3}}$, and that the equivalents of aluminium and phosphorus should be modified accordingly.

It may sometimes be interesting to compare certain properties of bodies by referring them to chemically equivalent weights, in cases where these correspond neither to molecular weights nor to the equivalents usually adopted, but we cannot conclude from this that those weights ought to be adopted as symbols of notations.

Apart from all these things, I agree with M. Berthelot that it is not advisable to exaggerate the importance of these questions, the solution of which cannot affect the important laws and theories of chemistry, and about which we can only reach a conclusion when we have arrived at more complete knowledge of the molecular constitution of compound bodies. This constitution itself will be doubtless revealed to us by researches on molecular mechanics, such as those on Thermo-chemistry, through which this eminent scientist aids so powerfully the advancement of science.

ART. XXIV.—*On some Reactions of Silver Chloride and Bromide;* by M. CAREY LEA, Philadelphia.

IN the course of some examinations made several years since on the reaction of silver bromide and iodide, I concluded that the dark substances produced by the action of light on these bodies are resolved by nitric acid into metallic silver which dissolves, and ordinary silver bromide and iodide, which remain. Since then I have met with a paper by Von Bibra on the reaction of silver chloride, in which that chemist found that darkened silver chloride was not attacked by nitric acid. My previous examination had not included the chloride, and the remarkable difference thus indicated, led me to return to the subject. I found my own previous results entirely confirmed as also Von Bibra's:—at least so far that only by the long continued action of nitric acid was there any decomposition of the darkened chloride, and even then, traces only of silver were taken up by the acid.

It therefore appears that the substance produced by the action of light on silver chloride is of a much more permanent character than in the case of the other silver haloids. Some other reactions noted in the course of the examinations which appear not devoid of interest, are given below.

Silver Chloride.

Our knowledge of the nature of the substances produced by the action of light on the silver haloids is very limited, owing to the fact that although the coloration is very intense, yet the portion of material acted upon is very small. Also because we have no means of removing from the mixture the unaltered haloid, except by agents that at the same time attack the altered substance, and completely change its character.

As to the first point—the small proportion of material actually altered by light. It is generally thought that silver chloride is reduced by the action of light to a sub-chloride containing half as much chlorine as the normal white chloride. Yet the loss of chlorine has been found too small to be weighed. Fresenius doubts if a loss in weight could be detected by the most delicate balance, and Von Bibra in the investigation above referred to, could not find the slightest loss in weight.

With a view to obtain some quantitative indication in the matter, the following determination was made.

Silver chloride was precipitated with an excess of hydrochloric acid, was well washed, and exposed to bright sunlight for five days. During this time it was spread in a very thin layer over the bottom of a large white porcelain basin, was frequently stirred up to bring constantly new surfaces to the light, and was kept moistened with water.

Of the resulting dark powder two grams were taken and were thoroughly treated with sodium hyposulphite to remove the unaltered chloride. Previous experience had shown that extraordinary precautions were necessary to effect this thoroughly, as the removal of the last portions of normal silver chloride is very difficult. Accordingly the strong solution of hyposulphite was many times renewed, each time being left to act for from twelve to twenty-four hours. Finally the gray residue (metallic silver) was washed, dried and weighed, and found to amount to twenty-one milligrams.

It thus appears that as the result of five days' action of strong sunshine, with frequent stirring up and mixing to bring fresh portions to the light, about one per cent only of the silver chloride was acted upon. And if we suppose this action to consist in removing one-half the chlorine, then the whole loss in weight by the action of the light should be but little over one-tenth of one per cent. This proportion is of course not inappreciable, and the observations of Fresenius and of V Bibra above quoted must be taken as referring to shorter exposures.

It was mentioned that another difficulty in verifying the nature of the action of light lay in the fact that those substances which can be used for dissolving out the unaltered chloride, also unfortunately attack the altered substance.

The two reagents most effectual for this removal are sodium hyposulphite, and liquid ammonia. That they also alter the blackened portion is at once evident from the fact that the residue which they leave behind instantly dissolves in cold nitric acid of sp. gr. 1·28 with evolution of red fumes, and is in fact, well known to be metallic silver, whereas before the application of these reagents, cold nitric acid has no effect, and even with boiling acid, applied for a long period only very slight decomposition ensues.*

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* The facts here mentioned lead to the curious reflection that the permanency of ordinary photographic prints is greatly diminished by the fixing process. For in this fixing process which consists in immersion into solution of sodium hyposul-

When cold nitric acid, sp. gr. 1·28, is poured over a quantity of the darkened chloride (precipitated with excess of HCl) and left to remain for a time in contact, this acid acquires no silver reaction whatever. It is therefore certain that by the action of light on silver chloride (precipitated in presence of excess HCl) *no metallic silver is reduced.* And as this black substance is quickly whitened by aqua regia, it is reasonable to conclude that the dark matter contain less chlorine than the normal, and is either a subchloride or an oxychloride. Beyond this, we know nothing with certainty.

When the dark substance was boiled for several minutes with the same nitric acid, no silver was extracted. But when the vessel was placed on a sand bath and kept at or near boiling point for eighteen hours, renewing the acid as it escaped, a distinct effect was produced. The substance became a little lighter in color, and the acid was found to have taken up enough silver to show a strong opalescence by addition of hydrochloric acid; not enough however to give an immediate precipitate.

Ammonia and sodium hyposulphite have this in common, that both leave metallic silver behind when the darkened chloride is submitted to their action. In the case of the sodium salt, it is of course understood that it is presented in strong solution and very large excess.

Silver Bromide.

Silver bromide was precipitated with excess of KBr and well washed, and exposed to light.

When cold nitric acid, sp. gr. 1·28, was allowed to stand for one minute over the darkened bromide it took up silver abundantly. Allowed to act for an hour at a heat considerably below 212° the color of the darkened bromide had considerably changed and at the end of seven or eight hours, complete decomposition had taken place. The resulting AgBr is lemon yellow and has more the general appearance of iodide than of bromide.

Philadelphia, Jan., 1878.

phite, an exceedingly stable substance which almost completely resists the action of boiling nitric acid is reduced to metallic silver, in the condition of a thin layer of almost infinitesimally fine particles, and in that condition not well able to resist external influences. To a considerable extent this difficulty is obviated by a partial substitution of gold for the metallic silver.

The use of a fixing treatment can of course never be dispensed with, as it is essential to remove the unaltered chloride. But it is evident that if a substance could be found which would remove the unaltered chloride only, without attacking that which has been darkened by light, a great advantage would probably be gained. The gold treatment could be dispensed with, the printing could be stopped when the right strength was obtained, without being carried further to allow for the weakening effect of the fixing agent, and the print obtained would probably be always perfectly permanent.

The fact that both sodium hyposulphite and ammonia in removing the unaltered chloride, reduce the altered to metallic silver, explains why the gold toning operation succeeds much better when applied after the fixing operation, than before it.

ART. XXV.—*On Journal Friction at Low Speeds;* by A. S KIMBALL, Professor of Physics in the Worcester Free Institute.

IN the Proceedings of the Royal Society, March 27, 1877, is published a paper by Professor Fleming Jenkin, on "Friction between Surfaces moving at Low Speeds," in which he establishes, by a series of very delicate experiments, conclusions at which the writer had previously arrived by a somewhat different course [see this Journal, March, 1876]. Professor Jenkin remarks, that in one case examined near the lower limit of velocity, "there was a slight and somewhat uncertain change of the opposite character, that is, a decrease of friction as the velocity decreased," and that "this case however, would require further examination." This phenomenon had been noticed in our laboratory, and an article had been written [see this Journal, May, 1877] embodying the results of our observations.

The effect of changes in velocity upon the coëfficient of friction is not an insignificant quantity, but has a sufficient magnitude to give it practical importance, in some cases at least. It therefore gives the writer great pleasure to be able to quote the experiments of so able an investigator, as confirming his conclusions, which are briefly these: The coëfficient of friction, at very low velocities, is small, it increases rapidly at first, then more gradually as the velocity increases, until at a certain rate, which depends upon the nature of the surfaces in contact and the intensity of the pressure, a maximum coëfficient, is reached. As the velocity increases beyond this point. the coëfficient decreases.

Later experiments have shown, what in fact might have been inferred, that from its maximum, the coëfficient decreases toward a constant value. The apparatus used in this series of experiments consisted of—1st. A pair of cone pulleys giving six changes of speed, driven by a shaft from the work shop. 2d. A rope transmission dynamometer, capable of indicating changes equal to one per cent of the largest, or five per cent of the smallest power transmitted. 3d. An experimental shaft with a pulley by which it was driven. The journals on this shaft were seven-eighths of an inch in diameter; they revolved in cast iron boxes three and one-half inches long, truly bored and polished. A thin cut was planed from the upper half of each box, so that when resting upon its journals it was not quite in contact with the lower half. Above each box was a lever of the second class, by means of which it was possible to apply any desired pressure to the revolving journals. The course of the experiment is obvious. The experimental shaft, loaded with a known weight, was driven at different speeds, the required power noted, and the coefficient of friction calculated. The results of seven series of experiments are shown in the following table, in which the first column shows the velocity of the circumference of the journal in feet per minute, the seven following columns giving the corresponding coefficients of friction. Since it was found impossible to maintain the same state of lubrication for any great length of time, every series which could not be completed in one half day was left unfinished.

TABLE I.

V.	*a.*	*b.*	*c.*	*d.*	*e.*	*f.*	*g.*
·59′	·186	·180	·187	·174	·170	·127	·138
2·17	·153	·150	·148	·131	·129	·114	·109
5·15	·122	·128	·122	·080	·080	·097	·083
10·99		·089	·086	·069	·067	·080	·060
19·71			·068	·058	·066	·052	·053
42·86			·068		·060	·045	·041

In spite of the irregularities to be seen in this table, the law as stated above is clearly shown. Some of the discrepancies are doubtless due to variations in the state of lubrication of the journals. The pressure on the lower boxes in series *a*, *c*, *d* and *e*, was 130 pounds; in series *b*, *f* and *g*, 210 pounds. In *a*, *b*, *c*, *d* and *e*, the journals were lubricated by wiping them with a handful of waste saturated with sperm oil. In *f* and *g*, as much oil was poured through the oil holes as the boxes would hold. To assist the reader in forming an idea of the value of the average results in Table I, Table II is given, which contains the separate readings from which series *c* was computed. Eight readings were made at each speed. The readings are the differences in tension of the two parts of the belt driving the experimental shaft:

TABLE II.

Speeds,	·59′	2·17′	5·15′	10·99′	19·71′	42·86′
Differences in lbs.	5·5	4·3	3·5	2·5	2·0	2·0
	5·4	4·2	3·6	2·6	1·9	1·9
	5·3	4·3	3·6	2·5	1·9	1·9
	5·4	4·4	3·5	2·5	2·1	1·9
	5·4	4·2	3·5	2·4	1·9	2·0
	5·5	4·3	3·5	2·3	2·0	2·0
	5·4	4·3	3·6	2·5	2·0	2·0
	5·4	4·3	3·6	2·5	1·9	1·9
Averages,	5·41	4·28	3·55	2·47	1·96	1·96
Coefficients,	·187	·148	·122	·086	·068	·068

The variation in observations taken at the same speeds, is nearly identical in every series with that shown in Table II.

In order to eliminate as far as possible gradual changes in the condition of the rubbing surfaces, the following order of observation was adopted in each series. Calling the slowest speed No. 1, four observations were made alternating

between Nos. 6 and 1.
Four " 1 " 2.
" " 2 " 3, etc.
" " 5 " 6.
" " 6 " 5, etc.
" " 2 " 1.
" " 1 " 6.

About 1,000 separate experiments were made under conditions of lubrication, and pressure upon the journals, purposely varied, so that the average result should represent as nearly as possible ordinary shop practice. Speeds as high as 100 feet (or 440 revolutions of the experimental shaft) per minute were employed. The results though not so uniform as those at lower speeds were decisive. The most probable values of the coefficient as shown by a graphical construction of all these experiments is shown in

TABLE III.

Speed,	1′	3′	5′	7′	10′	15′	20′	30′	40′	60′	80′	100′
Coefficient,	·150	·122	·104	·093	·079	·066	·058	·054	·053	·052	·051	·050

Thus we see that the conditions under which journal friction usually occurs are such that the maximum coefficient will be found at a very low speed. While using a journal 6″ in diameter with a load of only ten lbs., a reduction was made in the speed till the maximum coefficient (in this case ·27) was passed, after which the coefficient diminished very quickly to one-third of its maximum value. In the case of leather sliding on iron or wood where the intensity of the pressure is not great the coefficient will usually increase with the velocity. Examples of this will be found in the leather packings of hydraulic engines and in the slipping of belts upon pulleys.

It is usually assumed that the coefficient of friction is constant for all weights, not so light that the influence of adhesion is great in comparison, or on the other hand so great as to produce abrasion. An experimental examination of this point gave the results recorded in Table IV. Each coefficient is a mean derived from several observations, and though the results of experiments with constant weights were very uniform, it was found that the weights could not be changed without disturbing slightly the adjustment of the machine. These results seem to indicate that the coefficient of friction decreases slightly as the specific pressure on the journal increases even when the pressures are quite large.

TABLE IV.

Pressures are given in pounds on a square inch of longitudinal journal section.

P.	Coef'ts at ·59′ per min.		Coef'ts at 2·17′ per min.	
23·5	·154	·159	·136	·130
36·8	·157	·156	·138	·130
50·1	·149	·147	·119	·124
63·4	·149	·153	·117	·127
76·7	·153	·152	·108	·126
90·0	·151	·150	·109	·123
103·3	·155	·150	·111	·127
116·6	·149	·145	·113	·124
129·9	·147	·146	·112	·123
143·2	·145	·146	·113	·121
156·5	·147	·147	·113	·124
169·8	·146	·146	·112	·121
183·1	·145	·148	·109	·124
196·4	·144	·145	·111	·125

The larger part of the experiments referred to were made by Mr. Desper, a student of the Institute, whose patience and care deserve especial mention.

ART. XXVI.—*Observations of the Brightness of the Satellites of Uranus.*

[Communicated by Rear-Admiral John Rodgers, U.S.N., Superintendent U. S. Naval Observatory.]

THE following notes on the brightness of these bodies have been made during the regular series of measures for position. They are printed in order that they may be of service to any astronomer who may undertake an exact photometric determination of their brightness. The observations were made with the xxvi-inch telescope with powers of 600–900 diameters. The observers were H (Hall) and HN (Holden).

It was surmised by Professor Newcomb that the brightness of *Ariel* varied in different parts of its orbit (Wash. Ast. Obs. 1874, Appendix I, p. 43,) and that it was least bright in $p=180°\pm$ during 1874.

There have been observed in 1874–5–6–7 nineteen position angles of *Ariel:* of these, seven were 180°±, and twelve were 0°±.

A better test is the following: on fifteen nights this satellite was bright enough to allow of measures of distance. On six of these fifteen nights the position was 180°±, and on nine the position was 0°±. It would seem, therefore, to be more easily observed in one half of its orbit than in the other, so far as this evidence goes.

Dr. H. C. Vogel has suggested that *Titania* also is of varying brightness in different parts of the orbit. Early attention was given to this point. The following extracts from the observing books may be of use, when an exact photometric determination comes to be made. The weight indicates the steadiness of the images, five being perfectly steady. The positions given are to the nearest degree in p and nearest second in s.

1875. Jan. 26. *Titania*; $p=260°$, $s=19''$. Wt. 4. Titania is fainter than I have ever seen it.—HN.

1875. March 4. Four stars visible besides *Oberon* and *Titania*.

Oberon, $p=274°$ $s=26''$
Titania, $p=184°$ $s=35''$
Star 1, $p=350°\pm$ $s=80''\pm$
Star 2, $p=20°\pm$ $s=100''\pm$
Star 3, $p=130°\pm$ $s=130''\pm$
Star 4, $p=95°\pm$ $s=200''\pm$

Order of brightness; 4, 1, 3, 2; and 2 somewhat brighter than Titania.—HN.

1875. May 25. *Oberon*: $p=230°$, $s=31''$.
Titania: $p=21°$, $s=30''$.

Oberon very faint [and therefore as nothing is said of brightness of *Titania*], *Titania* brighter than *Oberon* at same distance.—H.

1876. Jan. 14.
$$\left.\begin{array}{l}\textit{Oberon}: p=11°,\ s=23'' \\ \textit{Titania}: p=48°,\ s=27''\end{array}\right\}\ \mathrm{Wt}=2.$$

Titania much brighter than *Oberon*. Moonlight.—HN.

1876. Jan. 20.
$$\left.\begin{array}{l}\textit{Oberon}: p=321°,\ s=27'' \\ \textit{Titania}: p=186°,\ s=34''\end{array}\right\}\ \mathrm{Wt}=2.$$

Measures of *Oberon* quite difficult [and of *Titania* not so.]—HN.

1876. Jan. 25.
$$\left.\begin{array}{l}\textit{Oberon}: p=181°,\ s=44'' \\ \textit{Titania}: p=352°,\ s=31''\end{array}\right\}\ \mathrm{Wt}=2.$$

Oberon easier to see [brighter] than *Titania*.—HN.

1876. Jan. 26. *Titania* in $p=310°$, $s=16''$. Wt. 5.

This is about the smallest distance at which *Titania* has been measured.

1876. Jan. 31.
$$\left.\begin{array}{l}\textit{Titania}: p=76°,\ s=17'' \\ \textit{Ariel}: p=29°,\ s=12''\end{array}\right\}\ \mathrm{Wt}=5.$$

Titania is at least twice as bright as *Ariel*. *Titania* is decidedly brighter than the satellite of Neptune. It is easier to see (under these conditions, Wt=5) within 16″ of Uranus, than the satellite of Neptune within 16″ of Neptune.—HN.

1876. Feb. 2.
$$\left.\begin{array}{l}\textit{Oberon}: p=388°,\ s=33'' \\ \textit{Titania}: p=7°,\ s=34''\end{array}\right\}\ \mathrm{Wt}=4.$$

After careful examination with 800 A and 400 A, I cannot decide which is brighter, *Oberon* or *Titania*, but if there is any difference, *Titania* is the brighter.—HN.

1876. Feb. 18. $\left.\begin{array}{l}\textit{Oberon: } p=228^\circ, s=30'' \\ \textit{Titania: } p=44^\circ, \ s=24''\end{array}\right\}$ Wt=4.

Titania brighter than *Oberon* but not very much so.—HN.

1876. Feb. 20. Star near *Uranus;* $p=89^\circ$, $s=27''$. Wt=1.

This star is about the brightness of *Oberon* and *Titania.* [For *Oberon* $p=276^\circ\pm$; for *Titania* $p=131^\circ\pm$].—HN.

1876. March 3. Two stars near Uranus, both slightly brighter than *Oberon.* $\left.\begin{array}{l}\text{Star 1, } p=230^\circ, \ s=60'' \\ \text{Star 2, } p=0, \quad s=50''\end{array}\right\}$ estimated.

Not much difference in the brightness of *Oberon* and *Titania.* [For *Oberon* $p=236^\circ\pm$, for *Titania* $p=268^\circ\pm$].

$\left.\begin{array}{l}\textit{Umbriel: } p=23^\circ, \ s=20'' \\ \textit{Ariel: } \quad p=173^\circ, \ s=13''\end{array}\right\}$ Wt=3.

Umbriel is more steadily seen than *Ariel* but not much more so. Then reduced aperture to fifteen inches. *Umbriel* was certainly seen and its position could have been measured. *Ariel* was not certainly seen. Sky hazy. Bright moonlight.—HN.

1876. Mar. 4. *Ariel:* $p=11^\circ$, $s=13''$. Moonlight and haze.

Ariel is quite as bright as last night in $p=180^\circ\pm$ under better circumstances.

$\left.\begin{array}{l}\textit{Oberon: } p=198^\circ, \ s=44'' \\ \textit{Titania: } p=177^\circ, \ s=32''\end{array}\right\}$ Wt=4.

Titania is brighter than *Oberon* although not much so.—HN.

1876. March 9. $\left.\begin{array}{l}\textit{Oberon: } \ p=57^\circ, s=27'' \\ \textit{Titania: } \ p=340^\circ, s=26''\end{array}\right\}$ Wt=3.

Titania is decidedly brighter than *Oberon.*—H.

1876. March 13. $\left.\begin{array}{l}\textit{Oberon: } p=347^\circ, s=38'' \\ \textit{Titania: } p=170^\circ, s=30''\end{array}\right\}$ Wt=2.

Oberon is brighter than *Titania.*—H.

1876. March 14. $\left.\begin{array}{l}\textit{Ariel: } p=23^\circ, \quad s=14'' \\ \textit{Umbriel: } p=176^\circ, s=19'' \\ \textit{Titania: } \ p=126^\circ, s=18'' \\ \textit{Oberon: } \ p=318^\circ, s=27''\end{array}\right\}$ Wt=2.

Ariel a little brighter than *Umbriel.* *Titania* very faint. *Oberon* brighter than *Titania.*—H.

1876. March 22. $\left.\begin{array}{l}\textit{Oberon: } \ p=78^\circ, s=23'' \\ \textit{Titania: } p=161^\circ, s=26''\end{array}\right\}$ Wt=3.

Titania much brighter than *Oberon.*—H.

1876. March 28. $\left.\begin{array}{l}\textit{Oberon: } \ p=42^\circ, s=33'' \\ \textit{Titania: } p=106^\circ, s=16''\end{array}\right\}$ Wt=3.

Oberon brighter than *Titania.*—H.

1876. March 31 $\left.\begin{array}{l}\textit{Oberon: } p=198^\circ, s=43'' \\ \textit{Titania: } p=152^\circ, s=24''\end{array}\right\}$ Wt=2.

Oberon brighter than *Titania.*—H.

It is not desirable to draw any conclusions from these observations as yet. They are here recorded as likely to be of service in photometric observations, and they will be continued as occasion offers.

ART. XXVII.—*A new method for the decomposition of Chromic Iron;* by EDGAR F. SMITH.

RECENTLY I was led to try the action of bromine and sodium hydrate upon pulverized chromic iron, and as the amount of chromium extracted in this manner was rather surprising, the following experiments were made, to ascertain what effect bromine alone would have upon the same substance.

I. Moderately fine chromic iron (·1500 grams) was placed in a tube of hard glass and after adding dilute bromine water and sealing the tube, the latter was placed in an air-bath and heated for twelve hours at a temperature about 180° C. When cool the tube was opened and its contents poured upon a filter. The insoluble residue was thoroughly washed by decantation, and upon the filter, with hot water. The filtrate after concentration was treated with a slight excess of ammonium hydrate, causing the precipitation of aluminum hydrate, etc. The latter was filtered off and the yellow-colored filtrate then warmed with hydrogen sulphide to reduce the chromic acid to oxide. The precipitate formed, after protracted digestion, was allowed to settle and the clear liquid filtered. After washing, the precipitate was dissolved in a few drops of dilute hydrochloric acid and re-precipitated. This operation was repeated and the precipitate finally transferred to a filter, washed, dried and ignited. The amount of chromic oxide found corresponded to 15·50 per cent of the substance taken.

The amount of chromium still remaining in the unattacked material was not estimated.

II. ·2000 grams substance, as finely pulverized as could be obtained by grinding the material in an agate mortar, were heated in a sealed tube with water saturated with bromine and a few drops of bromine. The tube remained in the oven four days, the temperature ranging from 175°–190° C. Upon opening the tube its contents were poured into a beaker and evaporated; water added and the solution filtered. The residual, unattacked mineral powder after washing, drying and igniting, weighed ·0820 grams. The filtrate from this was treated precisely as in (I) and the chromic oxide obtained from it amounted to 28·05 per cent.

III. In this experiment only ·1500 grams substance were employed. The material was of the same fineness as in (II). Instead of using dilute bromine water as heretofore an excess of bromine was poured over the substance and but a very small quantity of water added. For three days the tube was exposed to a temperature varying from 150°–175° C. At the expiration of this time the tube was examined, and as the substance appeared to be perfectly decomposed, the solution was removed from the tube and evaporated in a beaker to expel the large excess of bromine, upon the gradual disappearance of which a dark powder showed itself. The solution was strongly diluted with water and filtered. The insoluble residue was thoroughly washed with hot water. Dried and ignited, this weighed ·0140 grams.

The filtrate was mixed with an excess of ammonium hydrate and evaporated almost to dryness in a casserole. The solution was then diluted with water and filtered from the aluminum hydrate, etc., and treated as in (I). The percentage of chromic oxide extracted equaled 49·60 per cent.

IV. From the preceding experiments it appeared very evident, that all that was lacking to render the decomposition complete was to have the chromic iron in an exceedingly fine condition. To this end the material that had been ground to an impalpable powder in an agate mortar was elutriated, then dried and two distinct portions of ·1500 grams each placed in good hard glass tubes. To each portion was added a rather large quantity of bromine water and from ten to twelve drops of bromine. Both tubes were heated for one day at 130° C. For two successive days the temperature was maintained at 170° C. At the expiration of the third day, one of the tubes was removed from the oven and opened. Red oxide of iron had separated and undecomposed material was no longer visible. The whole was poured into a beaker and evaporated, water added and filtered. The residue was thoroughly washed, dried and ignited, then transferred to a beaker and heated with dilute hydrochloric acid. The entire mass dissolved readily and without a residue. The decomposition was, therefore, complete.

The filtrate from the iron oxide was evaporated almost to dryness after the addition of an excess of ammonium hydrate, then diluted and filtered. The solution was reduced with hydrogen sulphide and the precipitate, after filtering and washing, dissolved in dilute hydrochloric acid and re-precipitated with ammonium hydrate. This operation was repeated and the chromium oxide obtained was 62·66 per cent.

The second tube, removed not long after the first, contained a large amount of separated ferric oxide. This after filtering off the chromium solution also dissolved very readily in warm, dilute hydrochloric acid, leaving not the least trace of residue. The filtrate from this, after being similarly treated as above, yielded 62·83 chromium oxide.

These results accord with those of Garrett, who analyzed the same ore from Texas, Pa., and obtained about 63 per cent chromic oxide.

The ferric oxide that separates out in the tube during the decomposition will not contain any chromium, if it is thorougly washed with hot water. In no instance was iron found in the solution containing the chromic acid.

Several tubes containing the pulverized substance, potassium hydrate and bromine water were heated at 125° C., but invariably exploded before the decomposition was completed, and therefore no farther attempts were made to use the alkali to aid in the decomposition.

All that is necessary to effect the complete decomposition of chromic iron by this method is that the substance be exceedingly fine and that the same be exposed with bromine to a temperature of 180° C. from two to three days. The addition of ten to twelve drops bromine hastens the decomposition very decidedly.

In connection with the above it may be well to mention that the insoluble chromic oxide obtained by the ignition of the corresponding hydrate, may be brought into solution again by digesting it with bromine and sodium hydrate in a beaker.

Laboratory of the University of Pennsylvania, Dec. 22, 1877.

ART. XXVIII.—*Descriptions of two new genera of Pycnogonida;* by E. B. WILSON. *Brief Contributions to Zoology from the Museum of Yale College.* No. XXXVII.

Anoplodactylus, (nov. gen.)

Body slender. Rostrum cylindrical, rounded. Antennæ three-jointed, chelate. Palpi wanting. Ovigerous legs six-jointed, wanting in the male. Neck elongated, extending forward over the rostrum. Legs nine-jointed. Dactylus without auxiliary claws.

THIS genus differs from *Phoxichilidium*, which it otherwise closely resembles, in the number of articulations composing the ovigerous legs, and in the absence of auxiliary claws upon the dactylus. These claws, though comparatively small in *Phoxichilidium*, are very large in certain genera (e. g. *Pallene* Johnst.) They are movably articulated to the dactylus and furnished with a special set of muscles by means of which they are moved.

To this genus should be referred Kröyer's *Phoxichilidium petiolatum* (Voy. en Scand., Laponie, etc., Pl. 38, fig. 3) and probably also the *Phoxichilidium virescens* of Hodge.

Anoplodactylus lentus (sp. nov).

Phoxichilidium maxillare Smith, in Rep. on the Invertebrates of Vineyard Sound, p. 250, Pl. VII, fig. 35, 1874 (*non* Stimpson).

Oculiferous segment broad, as long as the two following segments taken together, not emarginate between the bases of the antennæ. Neck swollen. Posterior segment of body very slender. *Abdomen* rather more than twice as long as broad, slightly bifid at the extremity.

Oculiferous tubercle prominent, acute, placed on the anterior portion of the first segment. Eyes four, ovate, varying in color from light brown to black.

Rostrum very large, longer than the oculiferous segment, constricted at base, thus appearing somewhat clavate. The extremity is subglobose.

Antennæ hairy, long and slender, their bases closely approximated. Basal joint extending beyond extremity of rostrum. Chela stout, hairy. Dactylus very stout, smooth on margin.

Ovigerous legs stout, roughened by minute tubercles, the outer joints with many strong hairs, most of which are directed backward. The two basal joints are very thick; the first is shorter than its width, the second about twice the first. The succeeding joints are much more slender. The third is nearly two and a half the second, somewhat clavate, and suddenly constricted a short distance from the base. Fourth joint half the third. Fifth considerably less than fourth. Terminal joint much smaller than the preceding.

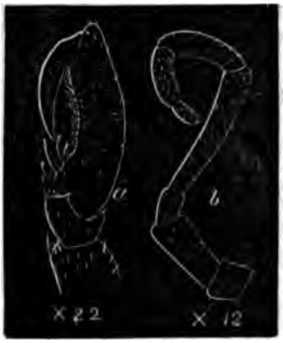


Fig. 1.—*Anoplodactylus lentus.* *a*, Terminal joints of leg; *b*, ovigerous leg.

Legs very long and slender. First and third joints very short, the second considerably longer and clavate. The three following joints are much longer, the sixth being the longest. The seventh or tarsal joint is very short and deeply emarginate exteriorly. Eighth joint (propodus) curved, with a rounded lobe near the base, which bears five or six strong spines. Exterior to these is a series of much smaller stout spines. The dactylus is stout, about two-thirds as long as the propodus. The whole surface of the body is scabrous. The legs bear a few scattered hairs, which are more numerous on the outer joints.

The genital orifices are situated on the lower side of the second joint of the legs, near the external margin. The sexes resemble each other closely except in the absence, in the male, of the ovigerous legs. The males are also, as a rule, slightly larger than the females. "It is most frequently deep purple in color, but gray and brown specimens are often met with." (*Verrill*).

This species is common in Vineyard Sound, but does not occur northward. It resembles the "*Phoxichilidium*" *petiolatum* of Kröyer, which has, however, according to the figures, the anterior segment much more slender and emarginate between the bases of the antennæ which are thus separated by a distinct interval, the posterior segment is represented as stouter and shorter, the proboscis more abbreviated, and the propodous of a different shape. Kröyer figures the ovigerous legs with seven joints, probably mistaking the constriction near the base of the third joint for an articulation.

Length of body in largest specimens (inclusive of rostrum and abdomen), 7 millimeters. Legs, 30 millimeters.

Pseudopallene, (gen. nov.).

Body robust. Neck broad and thick. Rostrum more or less acute. Antennæ three-jointed, chelate. Palpi wanting. Ovigerous legs composed of eleven joints. Legs nine-jointed. Dactylus without auxiliary claws.

This genus has not hitherto been distinguished from *Pallene*, and this has led to some confusion in the diagnosis of that genus.

In *Pallene*, as described by Johnston (Mag. Zool. and Bot., vol. i, p. 380), the ovigerous legs are nine-jointed, the neck is constricted and more or less elongated as in *Nymphon*, the rostrum is very short and nearly hemispherical, and the dactylus is armed with two very large auxiliary claws. A *Pallene*, to be hereafter described, collected by Prof. S. I. Smith at Wood's Holl, Mass., agrees in all these points with Johnston's *P. brevirostris*. The three species: *Pallene spinipes*, *P. intermedia* and *P. discoidea* figured in Gaimard's Voy. en Scand., Laponie, etc., do not belong to the *Pallene* of Johnston, but should probably be referred to *Pseudopallene*. Having seen no specimens of these species, I have been unable to verify this.

Pseudopallene hispida.

Pallene hispida Stimpson, Inv. of Grand Menan, p. 37, 1853.

Of this species there is but a single female specimen in the collection of the Peabody Museum, dredged by the United States Fish Commission in Johnson's Bay, near Eastport, Maine, in 12 fathoms rocky bottom. Stimpson records it from deep water off Grand Menan, "on *Ascidiæ callosæ*." His description being incomplete in several particulars, a full description is here given.

Body very broad, oval, neck not constricted. Oculiferous tubercle small, rounded. Eyes four, ovate, light brown. Oculiferous segment half as long as the body. Second and third segments with three prominent tubercles above, each of which is tipped by a hair. The lateral thoracic processes are very broad and are not separated by any interval; they bear on the outer margin two to four spine-like, hairy tubercles. Abdomen twice as long as broad, truncate, hairy.

Rostrum as long as oculiferous segment, with a constriction on each side below, giving it the appearance of being articulated at this point, acute-conical, with a rosette of filamentary processes around the terminal mouth.

Antennæ hairy, stout and swollen, about twice as long as the rostrum, tipped with amber color. Basal joints enlarged near their attachment. The second joint has a prominent rounded tubercle on the lower end, behind which the dactylus closes.

Ovigerous legs slender, eleven-jointed, terminal joint claw-like, trifid. Fifth joint somewhat clavate, considerably smaller than the fourth. The four outer joints are armed with three or four stout, smooth, curved spines.

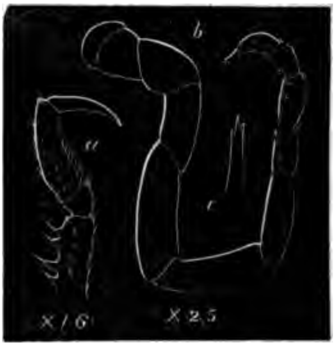


Fig. 2.—*Pseudopallene hispida.* a, Terminal joints of leg; b, ovigerous leg; c, terminal joint.

Legs very stout, the three basal joints short, overlapping each other in an imbricated manner. Fourth joint as long as the three basal joints taken together, much distended by the ovaries in the specimen described. Fifth, as long as the fourth but much more slender. Sixth, longer and more slender. Seventh (tarsus) very short, nearly triangular. Eighth slightly curved, armed with five or six spines on the inner (concave) margin. Dactylus slender, curved, acute, without accessory claws, about two-thirds as long as the preceding joint.

All of the legs bear more or fewer prominent, conical, spiny tubercles. These are arranged in longitudinal rows on some of the joints, particularly on the fifth and sixth, which appear deeply serrate on the external margin. The entire surface of the body is rough, and more or less hairy.

Genital orifices small, on the second joint of the legs.

Length (inclusive of rostrum and abdomen) 3 millimeters. Legs, 7·5 millimeters. Ovigerous legs, 3·7 millimeters.

ART. XXIX.—*Tantalite from Coosa County, Alabama, its mode of occurrence and composition;* by J. LAWRENCE SMITH, Louisville, Ky.

WHILE columbite has been long known from a number of localities in the United States and at some of them it is found in great abundance, the related mineral tantalite has never been identified, until recently I proved the fact of its occurrence in Alabama. Professor König has described (Proc. Acad.

Nat. Sci. Philadelphia, 1876) a mineral which he considered to be tantalite from Yancey County, North Carolina. There must however have been some mistake about the matter, for he states the specific gravity of the mineral to be 5·807. If this determination was given correctly this could not have been tantalite. I have found columbite from the North Carolina localities with specific gravities from 5·6 to 6·3, varying according to the amount of tantalic acid present with the columbic acid. There is no instance that I know of where tantalite has as low a specific gravity as 7.

I am indebted to Professor Eugene Smith, State Geologist of Alabama, for the specimen of tantalite that first came under my observation; he suspected it to be tantalite and sent it to me for verification; he had obtained the specimen from Judge Bently, to whom we owe what we know about the manner of its occurrence.

It is found in Coosa County, Alabama, detached from any rock, lying loose with "bowlders" (as Judge Bentley calls them) of granite more or less disintegrated. As, however, this region belongs to the older series of rocks (Professor Eugene Smith has not yet explored it) these blocks of granite are doubtless not bowlders, but detached masses, weather worn. They are found both under and on the surface for miles, running northeast and southwest. Across these in a direction northwest and southeast runs a ridge filled with quartz and flint rocks and at the intersection of the two, over about an acre of surface, some fifty specimens of tantalite have been collected, from the size of a pea to a lump one and a quarter pounds in weight.

Character of specimens.—They are irregular masses, without the slightest indication of crystalline form, just such pieces as I have obtained from the locality at Limoges; they are more or less rounded, with a ready cleavage in one direction; the specimens although long exposed, have undergone but little alteration, as indicated by examination made on several specimens. The specific gravity varied from 7·305 to 7·401.

On analysis it was found to consist of:

Tantalic acid,	79·65	81·62 metalic acids.
Tungstic "	1·10	
Stannic "	·87	
Manganese protoxide,	3·72	
Iron "	13·51	
Copper oxide,	·89	
	99·74	

The tantalic acid contains very little columbic acid.

Judging from the discoveries already made, and the large size of some of the pieces of tantalite, we may expect some important results from the future explorations in Alabama.

ART. XXX.—*On Amylidenamine Silver Nitrate;* by W. G. MIXTER. *Contributions from the Sheffield Laboratory of Yale College,* No. XXX.

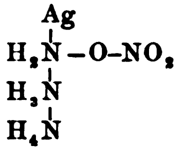
STRECKER (Liebig's Ann., cxxx, p. 220) states that silver nitrate produces in alcoholic solutions of valeralammonia, a white precipitate, which slowly turns black in the cold, but he says nothing as to the composition of the substance. I have obtained what is doubtless the same compound by the spontaneous evaporation of alcoholic ammonia solutions of valeralammonia to which an excess of silver nitrate had been added. The valeral was made from rectified commercial fusel oil by Parkinson's process. It did not give a constant boiling point. Different portions of it were treated with dilute ammonia water and the crystalline valeralammonia was washed on the pump with water. The product used for the first series of analyses was made by dissolving ten grams silver nitrate in 100 c. c. strong ammonia water and an equal volume of alcohol and then adding thirty grams of hydrous valeralammonia, from valeral boiling 95°–105°. Considerable oil separated which was dissolved by adding alcohol. The solution was exposed in a wide dish to the air until crystals ceased to form. The crystalline product was washed first with alcohol then with water, with alcohol again and finally with ether. It weighed, after drying over oil of vitriol, about six grams. The material for the second series of analyses was from thirty-four grams hydrous valeralammonia dissolved in 175 c. c. alcohol and 9·5 grams silver nitrate previously dissolved in 25 c. c. concentrated ammonia water. The crystals from this solution were washed as above described. The product weighed 12·5 grams. The filtrates in both experiments gave no more crystals until ammonio nitrate of silver separated, but gave a dark oil and some reduced silver. The carbon and hydrogen were determined by means of lead chromate, the nitrogen by Schiff's method and the silver by ignition.

	1st Series.	2d Series.	Calculated for $C_{15}H_{33}N_4O_3Ag$.
Carbon	42·11	42·26	42·35
Hydrogen	7·89	7·90	7·76
Nitrogen	13·24	13·18	13·18
Silver	25·26	25·19	25·41
Oxygen	[11·50]	[11·47]	11·30
	100·00	100·00	100·00

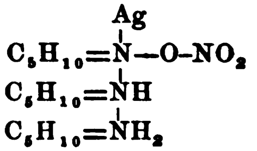
The equation $3(C_5H_{10}ONH_3)+AgNO_3=C_{15}H_{33}N_4O_3Ag+3H_2O$ doubtless represents the formation of the substance in question. It is not possible to give a gravimetric proof on account of some decomposition which occurs during the evaporation. The compound dissolves with but partial separation of silver in boiling water. The clear aqueous solution evaporated on a water bath leaves a slight black residue and a very soluble crystalline mass, which contains silver, reacts for ammonia and gives red fumes when heated with oil of vitriol. By distilling with ammonia water crystals are obtained in the distillate which reduce silver. Hot dilute acids decompose it with the separation of an oil which has the odor of valeral, and hot oil of vitriol evolves nitrous fumes from it. The reactions show that the substance contains the amyliden, ammonio and nitro groups and that it is an amine analogous to Rose's $3NH_3AgNO_3$, thus:

$$\left.\begin{array}{l}H_3N\\H_3N\\H_3N\end{array}\right\}AgNO_3 \qquad \left.\begin{array}{l}C_5H_{10}=NH\\C_5H_{10}=NH\\C_5H_{10}=NH\end{array}\right\}AgNO_3$$

The name amylidenamine silver nitrate is perhaps the best that can be given to the substance until more is known of its constitution. If the corresponding ammonio compound be regarded as diammonium-argentammonium nitrate (Graham-Otto, iii, 840) the derivative from valeralammonia may be regarded as di-amylidenammonium-argentamylidenammonium nitrate, thus:



$$\begin{array}{l}Ag\\ |\\H_2N-O-NO_2\\ |\\H_3N\\ |\\H_4N\end{array}$$



$$\begin{array}{l}\phantom{C_5H_{10}=}Ag\\ \phantom{C_5H_{10}=}|\\C_5H_{10}=N-O-NO_2\\ \phantom{C_5H_{10}=}|\\C_5H_{10}=NH\\ \phantom{C_5H_{10}=}|\\C_5H_{10}=NH_2\end{array}$$

Amylidenamine silver nitrate is insoluble in water, ammonia water, alcohol and ether, but freely soluble in ammoniacal alcohol from which it crystallizes. The crystals were too small and poorly defined for measurement. Hydrates do not appear to have formed. Dilute hydrochloric acid decomposes the body and the filtrate from the silver chloride formed yields on evaporation ammonium chloride. Hydrogen sulphide decomposes the amylidenamine silver nitrate when suspended in ether, with the formation of a black mass which contains sulphur, silver, a hydrocarbon or undecomposed substance and ammonium nitrate. The etherial solution after washing with water was evaporated and the thick yellowish oil remaining was dried over oil of vitriol. The following is the analysis of it:

		Calculated for.	
Carbon.........	64·99	C_{25}	65·79
Hydrogen......	11·25	H_{50}	10·97
Nitrogen.......	9·53	N_3	9·21
Sulphur........	14·01	S_2	14·03
	99·78		100.00

The results indicate a mixture of $C_{15}H_{30}N_3$ and $2(C_5H_{10}S)$. The oil has the odor of thiovaleral, and decomposes when distilled. Hydrochloric acid added to the concentrated etherial solution of it produces a white curdy mass which was not obtained free from adhering oil. This last product is soluble in alcohol, ether, and with the separation of an oil in hot water. Platinic chloride produces at once in the alcoholic solution a small light yellow precipitate and in a few minutes a dark brownish red curdy precipitate which yields to water, platinchloride of ammonium. The red precipitate after washing with water and alcohol, reacts for platinum, sulphur and a hydrocarbon. Lack of material prevented further investigation of the product from amylidenamine silver nitrate by the action of hydrogen sulphide.

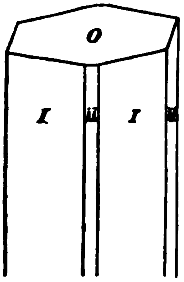
New Haven, January 10, 1878.

ART. XXXI.—*Note on the Crystallization of Variscite*; by ALBERT H. CHESTER.

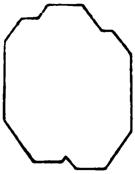
A MICROSCOPIC examination of certain small crystals of the mineral variscite from Arkansas reveals the following facts with reference to its method of occurrence and crystallization.

The crystals are rarely distinct, but are usually found in complicated groups, sometimes forming clusters of a sheaf form. Very rarely single prismatic crystals are found, sufficiently distinct to admit of measurement. Fig. 1 gives the most common form, belonging to the orthorhombic system, and showing faces of *I*, *ĭi*, *īi* and *O*. In this crystal $I \wedge I = 114° 6'$. In general but one termination is seen, but crystals showing both ends are sometimes found lying on the quartz matrix, the bases being similar to each other. The face *ĭi* is very small and therefore easily overlooked, and *īi* is about the same size as *I*, so that these crystals may readily be mistaken for hexagonal prisms. Crystals showing a simple termination like that in fig. 1 are seldom seen. More frequently the basal plane is like fig. 2, or still more complicated. These planes are often covered with a thin opaque white coating, probably of quartz.

1.



2.



A striking peculiarity of this mineral is its high lustre, like that of beryl, which it much resembles when viewed under a low power. The crystal figured above is 0·3 mm. in diameter, and is about the average size of those examined.

Hamilton College Laboratory, January 19th, 1878.

ART. XXXII.—*Discovery of a New Planet;* by C. H. F. PETERS. (From a letter to one of the Editors.)

IN the night of February 3–4, in revising one of my Zodiacal Charts, I found a star that I could not have omitted, as being of the tenth magnitude. Some measures therefore were immediately taken, which showed that the object is a planet hitherto unknown. Its position was,

Feb. 3, $13^h\ 46^m\ 47^s$ m. t. $\alpha = 10^h\ 1^m\ 38^s{\cdot}08.$ $\delta = +11^\circ\ 23'\ 34''{\cdot}1,$

from ten comparisons with Dm. +11°.2173,—the place of this star being determined by differentiation from LL. 19882, of which there are several modern determinations, viz: W. $10^h.91$, R. 3090, Arg. +11°.2190, Berlin Mer. Cir. in A. N., No. 1383.

Last night it clouded up, before the planet could be re-observed. But from the measurements of the preceding night follows the hourly motion $-1^s{\cdot}75$ and $+27''{\cdot}0$, or the daily motion -42^s and $+11'$, so that there will be no difficulty in finding the planet again, it having now already entered upon Chacornac's chart.

There has been some confusion of late in the numbering of planetoids, arisen from neglect of prompt communication. But it seems this new member will have to carry the number 180. If the priority of discovery remains to me, I propose the name *Eunike*, in commemoration of the glorious victories won by the Russian armies in their strife for humanity.

Litchfield Observatory of Hamilton College, Clinton, N. Y., Feb. 5, 1878.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *New mode of Determining Vapor Densities.*—NAUMANN has proposed an entirely new mode of determining vapor densities, founded upon the well established principle that when substances not miscible with a given liquid are distilled with this liquid, the quantity of the two bodies in the distillate, is, at a constant temperature of ebullition, in a constant ratio. Since, on the mechanical theory of gases, the vapor-tension, other things being equal, depends on the number of molecules which the vapor contains, the author sought a relation between the constituents of the distillate, expressed in molecular weights, and the vapor tensions of these constituents in the vapor mixture. The results confirm completely the hypothesis and establish the following law: The ratio of the quantities of the substances in the distillate, expressed in molecular weights, is equal to the ratio of the vapor-tensions of these constituents in the vapor mixture, mea-

ured at the temperature of ebullition. If g is the weight of one substance in the distillate, m its molecular weight and p its vapor tension at the boiling point t and under the barometric pressure b; and if G be the weight of the other constituent, M its molecular weight and P its vapor tension also at the boiling point, we have, by the above law,

$$\frac{\frac{g}{m}}{\frac{G}{M}}=\frac{p}{P}.$$

An extended series of experimental results are given, which establish the equality of these two ratios. From this equation it follows that $M=\frac{mGp}{gP}$. If one of the substances be water, $m=18$, p is obtained from Regnault's table of tensions of aqueous vapor, g and G are the quantities of the two bodies found by analysis in the distillate, and $P=b-p$. With naphthalene for example, distilled in steam, 49·4 grams of water and 8·9 grams of naphthalene went over at 98·2°, under a barometric pressure of 733 mm. For $t=98{\cdot}2°$, the tension of aqueous vapor $p=712{\cdot}4$ mm. and hence the naphthalene vapor tension $P=b-p=733-712{\cdot}4=20{\cdot}6$ mm. The molecular weight of water $m=18$. Substituting these values, $M=\frac{18\times8{\cdot}9\times712{\cdot}4}{49{\cdot}4\times20{\cdot}6}=113$. The formula $C_{10}H_8$ requires 128. This is an extreme case, but the result shows that the above formula, and not any multiple or submultiple of it, is correct. The method is capable of indefinite extension.—*Ber. Berl. Chem. Ges.*, x, 2098, Jan., 1878. G. F. B.

2. *On a new Oxide of Sulphur, Persulphuric oxide.*—BERTHELOT has described a new oxide of sulphur which he calls persulphuric oxide, corresponding to perchromic and permanganic oxides. It may be obtained by the action of the silent electric discharge, under a high tension, upon a mixture of equal volumes of absolutely dry sulphurous oxide and oxygen gases. In solution as persulphuric acid, it is produced in the electrolysis of concentrated sulphuric acid, and has been confounded with the so-called antozone. It is also formed when hydrogen peroxide is mixed with concentrated sulphuric acid. The oxide is best prepared with the concentric tube apparatus,* by the action of electricity as above. At the end of eight to ten hours, the walls of the annular portion are covered with drops of a thick adherent liquid. Exposed to a temperature of 0°, it crystallizes sometimes in granular indistinct crystals, sometimes in transparent thin and flexible needles, many centimeters long and of an appreciable breadth, often crossing the tube. It resembles sulphuric oxide, but the latter is opaque, in finer needles, shorter and narrower. It possesses a considerable vapor tension and sublimes spontaneously in the

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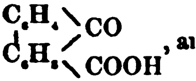
* Ann. Chim. Phys., V, xii, 463, December, 1877.

tubes containing it. Its composition was determined by synthesis and by analysis. Synthetically, the residual gas after the action was withdrawn by a mercury pump and measured; it was one-eighth of the original gas, corresponding to the equation $(SO_2)_2+O_4=S_2O_7+O$; or in volumes 4+4 originally and 1 finally. Analytically by opening the tube under a titered stannous chloride solution, the excess of oxygen was determined; and by precipitation with barium chloride, the sulphuric acid was ascertained. The ratio obtained was $SO_3 : O :: 10 : 1$; the theoretical ratio $S_2O_7=(SO_3)_2 : O$ or 160 : 16. The same result was reached by simple weighing, the persulphuric oxide formed having a weight of 104 and the constituent sulphurous oxide and oxygen a weight of 94·1+10=104·1, proving it to consist only of sulphur and oxygen. Persulphuric oxide thus prepared may be preserved several days near 0°, but at the end of a fortnight it begins to decompose. Its aqueous solution rapidly loses oxygen, and its solution in concentrated sulphuric acid also loses oxygen but more slowly. It fumes in the air. Its solution in sulphuric acid evolves oxygen rapidly in presence of platinum sponge. Treated with sulphurous acid, hyposulphuric acid is formed. Barium hydrate gives barium persulphate, which is soluble in water.—*C. R.*, lxxxvi, 20, January, 1878. G. F. B.

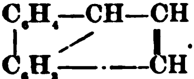
3. *On the Formation of Ammonium Nitrite in Combustion.*—Zöller and Grete have reëxamined the assertion of Schönbein that ammonium nitrite is formed by combustion, deeming the statements of Carius and Berthelot that ready formed ozone will not oxidize nitrogen irrelevant, since the position of Schönbein is that the nitrite and the ozone are simultaneous products. They found that water which had been produced by the combustion of hydrogen in atmospheric air gave a distinct reaction both with Nessler's test and with potassium iodide, starch and a few drops of acid. Numerous experiments with pure hydrogen—a large gasometer full being burned every day for two months—and pure air, with pure hydrogen and common air, and with unpurified hydrogen, and gave always water containing nitrite. Two to three hundred cubic centimeters of this water distilled from pure baryta, gave a distillate in which platinic chloride produced a precipitate. —*Ber. Berl. Chem. Ges.*, x, 2144, Jan., 1878. G. F. B.

4. *On Fluoranthrene, a hydrocarbon from Coal tar.*—In examining for phenanthrene, a solid hydrocarbon of high fusing point, obtained from coal tar, Fittig and Gebhard found that none of this hydrocarbon was present, but that the mass consisted of pyrene and a new hydrocarbon analogous to it. The two were separated by means of their picrates. The new hydrocarbon is more soluble in alcohol than pyrene, crystallizes in large brilliant plates, which fuse at 109°, and has the composition $C_{15}H_{10}$. The picrate crystallizes from alcohol in long reddish-yellow needles fusing at 182°-183°. On oxidation, it yields a mixture of a quinone and an acid, the latter monobasic, with the formula $C_{14}H_8O_2$. Heated with zinc dust, it gives fluorene, and with lime, diphenyleneketone.

Hence the acid is diphenyleneketonecarbonic acid, with the formula

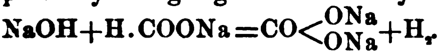


, and the hydrocarbon has the constitutional formula



. The authors give the hydrocarbon, therefore, the name fluoranthrene, calling attention to the fact that Schmitz has proved the identity of Barbier's fluorene with diphenylenemethane. In a postscript, Fittig says that he has just seen the paper of Goldschmiedt upon idryl, and has no doubt that the hydrocarbon $C_{15}H_{10}$ there described is identical with his fluoranthrene.—*Ber. Berl. Chem. Ges.*, x, 2141, Jan., 1878. G. F. B.

5. *Simple synthesis of Formic acid.*—MERZ and TIBIRCA have studied the reaction of carbonous oxide upon caustic alkali, observed first by Berthelot, and find that when a current of this gas is passed over soda-lime at a temperature of about 200° to 250°, the formation of formic acid is rapid. Under favorable conditions an extremely rapid current of gas is completely absorbed, forming an instructive class experiment. This synthesis is important in view of the fact that the hydrogen solidified by Pictet was prepared by fusing together sodium hydrate and formate



$NaOH + H.COONa = CO{<}{\substack{ONa \\ ONa}} + H_2$. The fusion took place in the generator.—*Ber. Berl. Chem. Ges.*, x, 2117, Jan., 1878. G. F. B.

6. *Constitution of Nicotine.*—LAIBLIN has taken another step toward unraveling the constitution of the alkaloids by successfully oxidizing nicotine by means of potassium permanganate, obtaining thereby an acid which he calls nicotinic acid, in colorless needles, fusing at 225°-227° and yielding on analysis the formula $C_6H_5NO_2$, identical with the acid obtained by Weidel by oxidizing with nitric acid. On distillation with lime, the salts of this acid yield pyridine. Hence the acid is pyridine-carbonic acid C_5H_4N—·—COOH.—*Ber. Berl. Chem. Ges.*, x, 2136, Jan., 1878. G. F. B.

7. *Chemical Philosophy.*—As recent discussions in the French Academy, together with the paper of Marignac, translated in the last number of this Journal, and the correspondence between Berthelot and Marignac which appears in the present number, may tend to unsettle opinions in regard to the fundamental principles of modern chemistry, it seems important to rehearse in few words certain considerations, which do not appear to have been duly weighed in these discussions. The modern theory of chemistry assumes the recognition of molecules as definite magnitudes, and the law of Avogadro as an accepted principle. It does not attempt to justify these assumptions by chemical evidence, which is confessedly inadequate, but it accepts the principles as definite results of molecular mechanics. This department of physics involves the mechanical theory of heat and the kinetic theory of gases, which have been developed by such masters as Rankine, Clausius, Joule

and Maxwell, and it is not too much to say that these theories—essentially one and the same—rest to-day on as firm a foundation as the undulatory theory of light, and the molecule is a vastly more reasonable conception than that indefinitely attenuated yet indefinitely elastic medium we call the ether. Of the kinetic theory of gases the principle of Avogadro is a necessary consequence and must be true unless this whole theory is a delusion. It is not however at present important to consider whether the absolute truth of the theory will probably be hereafter vindicated or the reverse. For the moment the only question is whether the kinetic theory of gases is a suitable basis for the system of modern chemistry, and we are surprised that any scholar, who appreciates the present position of the theory should not acquiesce in the decision in which the great body of working chemists—certainly of those outside of France—agree. The two great uses of systems, or theories, in science are to direct investigation and to facilitate the acquisition of knowledge. Of the value of the molecular theory in chemistry the history of investigation during the last ten years is sufficient evidence for the first point, and for the second point the paper of Marignac above referred to furnishes abundant indications of the confusion, which would necessarily attend any attempt at the present day to base a system of chemical notation solely on the early crude conceptions of combining proportions or chemical equivalents. As we have written in another place, "Our modern science is a philosophical system based on ideas distinctly stated and consistently developed. . . . . . Of course we are far from believing that the ideas now prevailing are necessarily true, and it is perhaps to be expected that our modern school will share the same fate as that which preceded it, but we do believe that the coming system, whatever it may be, will be based on equally clear conceptions, and that in attempting to clarify our ideas and realize our conceptions, we are following the right path and making the only satisfactory progress."

As is well known, although the kinetic theory of gases defines molecular magnitudes, it establishes only their *approximate* relations and even these in but comparatively few cases. It thus gives however the first terms of proportions, whose remaining terms can be supplied by purely chemical evidence, chiefly that obtained by the study of chemical reactions and by quantitative chemical analysis. For a limited number of substances the relative molecular weights as deduced by chemical methods can be brought into comparison with those directly given by the molecular theory, and the agreement is all that could be expected. Indeed, considering the uncertainties which both the physical and the chemical methods involve, and our great want of knowledge of the changes of molecular structure, which evidently very constantly attend the volatilization of a solid substance, it is surprising that the discrepancies are not more numerous than those which have as yet appeared; and to abandon our clear conceptions and hitherto safe guides on account of a few anomalies, which remain to be explained, would be as foolish as for a navigator to throw away his chart and compass because he could not fully account for all the deviations of the magnetic needle.

In a recent paper, of which a notice will be found in the previous number of this Journal, M. Berthelot, after referring to the well known principle that the tendency of a chemical system is toward that composition which corresponds to the maximum evolution of heat, writes—"the secret of the decomposition of baric perhydrate is not to be found in any symbolic considerations drawn from a figurative arrangement of atoms, but is explained by very simple and very obvious principles, resulting from the regular action of molecular mechanics." The tone here taken in regard to the study of structural chemistry, which was also heard in the debate in the French Academy, greatly surprises us. Certainly these eminent French chemists must know that in Germany the study of chemistry has been during the last ten years almost solely directed by theories of molecular structure, and that during this period the German chemists, thus guided, have done more to advance their science than all the other nations of the world combined. Moreover, if there are such things as molecules, and if the present science of "*molecular mechanics*" is a legitimate branch of knowledge, then the so-called "Law of Avogadro" is an established principle, and then also molecular structure is a necessary inference at least in the only sense in which it is held by the great masters of modern chemical philosophy.

J. P. C., JR.

8. *The Laws governing the Formation of Ethers.*—N. MENSCHUTKIN recently presented to the Russian Chemical Society a paper of great general interest, as it is one of the first fruits of the comparative method of research which has already yielded such brilliant results in the hands of Bogusky. His work relates to the formation of the acetic ethers from sixteen different alcohols, and is in part a revision of that of Berthelot and Pean de St. Gilles, who were the first to enter this field, but whose researches, as the science was not ready for them, have lain neglected since 1863. The position of the hydroxyl group, he finds, has a marked effect on the etherification, the rate during the first hour being from twenty to thirty per cent faster, during the following hours somewhat slower for primary alcohols (characterized by CH_2OH) than for secondary (characterized by $CHOH$), while the limit, that is, the whole amount of ether which it is possible to form from secondary alcohols, varying from 61·5 to 66·65 per cent of the theory, is lower than that of the primary, 69·61 to 82·24 per cent; differences of molecular weight within an homologous series do not affect the initial rate, which is the same for all its members with a few easily explained exceptions, but do influence the limit, which increases about two per cent in the primary series, about 1·1 per cent in the secondary with each increase of CH_2; the initial rate and limits of unsaturated alcohols are lower than those of the saturated, the rates differing by about ten per cent, and—the

most curious fact observed in this research—benzyl and cinnamyl alcohols have the same initial rate as allyl alcohol in spite of the wide differences between the aromatic compounds and those of the fat series, which leave little in common to these substances except their alcoholic nature: it follows from these facts that new alcohols can easily be referred to their proper class by determining their initial rate of etherification. Menschutkin's somewhat lengthy discussion of the relative rate, i. e., the rate referred to the limit seems rather unnecessary, as it is of course a mere function of the limit, but it is not worth while to dwell on the single blemish in a paper so full of interesting observations, many of which cannot even be mentioned in this notice from want of space, and of valuable suggestions for future research, notably those in regard to the bearing of isomerism on the rate and limit, which it is to be hoped he will follow out thoroughly, as soon as he has finished the tertiary alcohols (containing COH) with which he is now at work.—*Ber. Deutsch. Chem. Ges.*, x, 1728, 1898. C. L. J.

9. *Solid Hydrogen, Density of Liquid Oxygen.*—With the apparatus described in the last number of this Journal, M. PICTET has succeeded in both liquefying and solidifying hydrogen, and has obtained a product which is described as having a metallic appearance, and justifying the opinion long since expressed by Dumas, that hydrogen is a *gaseous metal*. M. Pictet prepared the hydrogen by heating potassic formiate with potassic hydrate in the "shell" of his apparatus (page 139). The pressure increased gradually and in a little more than half an hour it reached 650 atmospheres. At this point the pressure remained stationary for some seconds, when "the stop-cock was opened and a jet of a steel-blue color escaped with a hissing noise." "This jet suddenly became intermittent, and the spectators observed a hail of solid corpuscles projected with violence on the ground, where they produced a crackling noise."

M. Pictet also announces the result of a rough estimate of the density of liquid oxygen based on many factors, but *chiefly* on the dimensions of his apparatus, and "the variation of pressure corresponding to the condensation of oxygen in the tube immersed in the carbonic acid." This variation corresponded to 74·26 atmospheres, and from this he deduces that the tube whose volume was 46·25 cubic centimeters held 45·467 grams of the liquid gas. Assuming that the tube was full, the density of liquid oxygen must be nearly the same as that of water, and if, as is probable, the tube was not quite filled, the agreement is still closer. This density is about one-half of that of sulphur, and the result is regarded by Pictet as a most complete experimental verification of a prediction of Dumas based on the inference that the atomic volumes of oxygen and sulphur must be equal when in the same state of aggregation. As the atomic weight of oxygen is one-half of that of sulphur, Dumas had concluded that the densities of the two substances would bear to each other the same ratio.

In Nature for Jan. 31 may be seen a large wood-cut representation of the apparatus used by M. Caillelet in his experiments described in our last number. J. P. C., JR.

10. *The effect of Light and Heat upon the electrical resistance of Selenium.*—W. SIEMENS has undertaken a long research upon the photo-electric properties of selenium and concludes that its sensitiveness to light is a peculiar property of this metal, and finds that it is not a general property of metals as has been claimed by Bornstein. Siemens points out the use of selenium as a photometer for lights of all colors. The photometers now in use do not admit of the comparison of colored lights with much precision. When we pronounce that two lights of different colors are equal, our decision is a subjective one. The selenium photometer, however, gives unmistakable indications for the strength of lights of all colors. Unfortunately there is no means at present of graduating and obtaining the values of the scale divisions of such a photometer. Siemens has endeavored to obtain an empirical scale, but without much success. He hopes, however, that other observers with better means may be more successful.—*Ann. der Physik und Chemie*, No. 12, 1877, p. 521. J. T.

11. *On the Influence of Light upon the electrical resistance of Metals;* by G. HAUSEMANN.—The author has submitted the results of Bornstein upon the above subject to proof. He used a very sensitive galvanometer and a thermo-electric element of iron and copper from which, by keeping the ends at a constant difference of temperature, he was enabled to obtain a constant electromotive force. When the metals were intercalated in the circuit and their resistance measured, no difference was observed which could be attributed to the effect of light. Another method depending upon damped vibrations was used with the same result, and Hausemann therefore concludes that Bornstein is in error in attributing the effects he obtained to light. In another paper he discusses the results obtained by Bornstein in photo-electricity and fails to confirm the results of that investigator.—*Ann. der Physik und Chemie*, No. 12, 1877, p. 521. J. T.

12. *On Electromagnetic and Calometric absolute measurements.*—Prof. H. T. WEBER in a paper not yet concluded, discusses the various results, obtained by different observers for the value of Siemens' resistance unit, and concludes from three independent sets of measurement that the divergent results obtained by MM. Wilh. Weber, F. Kohlrausch and L. Lorenz, are affected with errors of observation. Prof. H. T. Weber finds for the value of Siemens' resistance unit the expression

$$1 \text{ S. M. U.} = 0{\cdot}9550' \times 10^{10}\left(\frac{\text{millim.}}{\text{sec.}}\right)$$

Weber concludes his first paper as follows:—

"(1.) The fundamental laws hitherto recognized of induced currents of variable intensity represent with great precision the real facts. The opinion of M. Lorenz, that the great difference between the results found by M.M. Weber, F. Kohlrausch and the physicists of the British Resistance Committee was the consequence of our imperfect knowledge of the laws of such induced currents, finds no confirmation at all in the above experiments.

(2.) Absolute measurements of resistance can with the means of galvanic observations nowadays at our disposal, be carried out with such exactness and certainty as can only be attained in few departments of physics. The notion widely accepted among physicists, that absolute measurements of resistance belong to those physical measurements which are capable of giving only roughly approximate values, and require peculiarly equipped localities for carrying them out—an opinion to which W. Siemens, among others, has given expression in the sentence 'we may certainly pronounce positively that even the most practised physicists, supplied with the most perfect instruments and localities, will not be able to make absolute resistance measurements that would not differ by some percentage'—is refuted by the above results of experiment. According to my experience, these measurements can be effected with tolerable accuracy with very scant means and in moderately equipped localities."—*Phil. Mag.*, Jan., 1878, p. 30. J. T.

13. *Notes from the Chemical Laboratory of the Johns Hopkins University*, Nos. 4–8. 22 pp. 8vo. Baltimore, December, 1877.—No. 4, on the oxidation of Bromethyl-toluene and of similar substitution-products, by H. N. MORSE and I. REMSEN; No. 5, on the oxidation of xylenesulphonic acids, by M. W. ILES and I. REMSEN; No. 6, on acetylamidophenols, by H. N. MORSE; No. 7, a correction, by I. REMSEN; No. 8, a Lecture-experiment, id.

II. GEOLOGY AND MINERALOGY.

1. *Annual Report of the Wisconsin Geological Survey for the year* 1877; by T. C. CHAMBERLAIN, Chief Geologist.—Besides valuable notes on the local geology of portions of the State, this Report contains brief descriptions of new Silurian fossils of Wisconsin. The Potsdam sandstone afforded the species *Bellerophon antiquatus*, and new species of each of the following genera of Trilobites: *Conocephalites*, *Crepicephalus*, *Ptychaspis*, *Agraulos*, *Arionellus* and *Elliptocephalus*; and the Lower magnesian limestone trilobites, of the genera *Dicellocephalus* (near *Bathyurus* of Billings) and *Illænurus*, beside *Euomphalus*, and species of *Scævogyra*, *Metoptoma* and *Leptæna*. A number of new species also are described from the Trenton, Hudson River and Niagara groups. The Report closes with a biographical notice of Moses Strong, who had been one of the assistant geologists of the Survey.

2. *Geological Survey of New Jersey.* Annual Report of GEORGE H. COOK, State Geologist, for the year 1877.—The final report on the clay district of Middlesex County, states that the yield of clay in some parts is 10,000 tons an acre, and single acres have yielded 40,000 tons; that in a single year 265,000 tons of fire clay and 20,000 of stone-ware clay have been dug; and there is no reason why this amount should not be quadrupled.

Professor J. C. Smock gives a valuable account of the drift of New Jersey, with the courses of the glacial striæ. The direction is *southwest* on the western slope of the Blue Mountain in Sussex and Warren Counties, at the zinc vein of Mine Hill in Franklin, on the west slope of Palisade Mountain at Bergen Hill, on First and Second Mountains west of Orange, and some other points; it is *southeast* on the top of Palisade Mountain at various points from Alpine to Newark and mostly between S. 15° E., to S. 25° E.; and nearly east-and-west (S. 60° E. to S. 80° E.) on the western slope of Blue Mountain along Greenville turnpike, N. Y., at High Point, Blue Mountain, on eastern slope of Blue Mountain southwest of Branchville, and on First and Second Mountain west of Paterson. The southern edge of the drift in New Jersey is stated to pass along the line of Short Hills extending from Perth Amboy, on the north side of the Raritan, to the First Mountain. Between First and Second Mountains it fills the valley for less than half a mile south of the Morris and Essex railroad. It may thence be traced to Long Hill, Morristown, Dover and on to the Delaware below Belvidere. "The whole line of this moraine is remarkably plain and well defined."

3. *Bulletin of the United States Geological and Geographical Survey;* F. V. Hayden, Geologist in charge. Vol. iv, No. 1.—This number of the Bulletin contains papers by G. B. Sennett on the Ornithology of the Rio Grande; by E. D. Cope on Cretaceous and Tertiary fishes, and on Professor Owen's remarks on the Pythonomorpha; by V. T. Chambers on new Tineina, etc., from Texas; by A. R. Grote on Noctuidæ, chiefly from California; by J. S. Kingsley on N. American species of Alpheus; by Dr. C. E. McChesney on the Mammals of Fort Sisseton, Dakota; by R. Ridgeway of the American Herodiones; by S. H. Scudder on the Butterflies of Southern Utah and Arizona; by Drs. Coues and Yarrow on the Herpetology of Dakotah and Montana; by Dr. E. Coues on the Consolidation of the hoofs in the Virginian deer, and on a breed of solid-hoofed pigs apparently established in Texas.

In the case of the deer the consolidation is confined to the horny substance of the true hoof.

With regard to the pigs of Texas, the *animal is completely solidungulate*, and the anchylosis of the terminal phalanges of the toes has produced a single broad phalanx in the axis of the limb. Above this, however, the other two phalanges, medial and proximal, of each of the two principal digits, remain perfectly distinct. The hoof is perfectly solid and on its sole there is a broad angular elevation of horny substance which is curiously like the frog of the horse's hoof. The breed is so firmly established that no tendency to revert to the original and normal form is observable. Mr. G. W. Marnock, of Helotes, Bexar Co., Texas, who sent specimens and facts to Dr. Coues, states that in the cross of a solid-hoofed boar with a sow of the ordinary type, a majority of the litter have the peculiarity of the male parent.

Dr. Coues states that there is here a change from an artiodactyle (or even-toed) mammal to a perissodactyle (or odd-toed). This is true if we look only at the literal meaning of the word perissodactyle. But in actual structure, the animal is no less artiodactyle than before; for the two toes are still present as much as before, although coalesced at extremity; and the *fourth*, the *outer* of the two, *equals the third*—which is the grand characteristic of artiodactyles and determines that even stroke of the foot to which is due the possibility of the coalescence of the two metacarpal or metatarsal bones into a "cannon" bone. J. D. D.

4. *Report on the Geographical and Geological Survey of the Rocky Mountain Region;* by Major J. W. POWELL, in charge of the survey. 20 pp., 8vo.—This report is a brief and very general statement of the work done by Major Powell's expedition in 1876. The region investigated lies mainly within the Territory of Utah. The work of the primary triangulations was under Professor A. H. THOMPSON and the topographic in two parties, under Mr. W. H. GRAVES and Mr. J. R. HENSHAWE. Geological investigations were carried forward by Major POWELL, Mr. J. K. GILBERT and Captain C. E. DUTTON. The results also include researches in the departments of Botany and Ethnography, besides the taking of photographic views, and an investigation of the climate of Utah with reference to the rise and fall of Great Salt Lake.

5. *On the variations of the Decorticated Leaf-scars of certain Sigillariæ; and on the variations of the Leaf-scars of Lepidodendron aculeatum* Sternberg; by HERMAN L. FAIRCHILD.—These two papers from the Annals of the N. Y. Academy of Science, are illustrated by several excellent plates, showing the variety of forms which occur in the markings of one and the same species.

6. *Elements of Geology;* a text-book for Colleges and the general reader; by JOSEPH LECONTE, author of "Religion and Science," and Professor of Geology and Natural History in the University of California. 588 pp. 8vo, with numerous wood cuts.—Professor LeConte is well known to the readers of this Journal through his papers on binocular vision, the glaciers of the Sierras, the lava floods of the Cascade Mountains and other parts of the Pacific slope, a theory of the origin of the earth's features, and on a theory with regard to periods of catastrophe being "critical periods" in the development of species, besides some other topics. His work on Geology opens with the Dynamical department of the science, taking up first the subjects of atmospheric and aqueous agencies and continuing with those of volcanoes and earthquakes, and gives with ability and clearness the facts and principles. Part II, on Structural geology, treats of the structure and positions of stratified and igneous rocks, the arrangement and condition of fossils, the classification or chronological order of strata, mineral veins, and the structure, origin and erosion of mountains. Part III is occupied with Historical geology. The illustrations are numerous and well selected.

7. *Contributions to the Fossil Flora of the Western Territories.* Part II. *The Tertiary Flora;* by LEO LESQUEREUX. 1877. pp. 366, and 65 lithographic plates, 4to.—This noble and important volume, one of the latest and ripest fruits of Dr. Hayden's Geological and Geographical Survey, is just issued, and at this moment we can only announce its publication. A critical account of it may in due time be given, either from the paleontological or botanical point of view, or from both.

Also the paper on *Land Plants* recently discovered *in the Silurian rocks of the United States*, published in the Proceedings of the American Philosophical Society, of October last (with a plate) deserves particular mention. The discovery of some of these remains was recorded in this Journal, for January, 1874; but new materials and new studies have confirmed Mr. Lesquereux in his opinion of their character. Meanwhile Count Saporta has reported the finding of a branch of a Fern in the Silurian slates of Angers, France, announcing at the same time the prior American discovery. A. G.

8. *The rocks of the "Chloritic formation" on the Western border of the New Haven region;* by GEORGE W. HAWES.—In a contribution from the Sheffield Laboratory* on the above named subject, the writer gave analyses of several specimens of rock from the so-called "Chloritic formation," and pointed out the resemblance in composition between these rocks and basic igneous rocks. By the aid of the microscope the rocks were inferred to be pyroxenic. On a further examination with better facilities, I have found that the mineral, supposed to be pyroxene, is hornblende, and that the rocks belong therefore to the diorite group. These rocks present many very interesting microscopic peculiarities, among which are some that distinguish them very well from the eruptive diorites, and I refer those who are interested in the subject to the forthcoming volume of the New Hampshire geological survey, in which analogous rocks are discussed and illustrated, and in which a revision of my previous study† upon these rocks will be found.

9. *Elevated Quaternary beds of Grinnell Land and North Greenland.*—The December number of the Annals and Magazine of Natural History contains an article on these evidences of former Arctic subsidence and elevation, by H. W. FEILDEN, naturalist to the late Arctic Expedition. The author speaks of his communication as a supplement to a paper on the same subject by Dr. Gwyn Jeffreys, in the September number of the same Journal; and he alludes also to a memoir by Mr. Henry H. Howorth, published in the Journal of the Royal Geographical Society for 1873, in which the subject of the elevation of the land in the Arctic Regions is "treated in a very able and comprehensive manner." He states that his observations sustain the opinion of Mr. Howorth, that "the land which surrounds the North Pole is undergoing a general movement of upheaval; or, to be perfectly correct,

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* This Journal, Feb., 1876. † Ibid., Aug., 1876.

we find on it, in all directions, evidences that there has been a movement of upheaval since there was any subsidence." In Grinnell Land and North Greenland shell beds in some localities rest on "Miocene strata, and extend to an elevation of not less than a thousand feet above their level," proving, as he states, that since the Miocene there has been "a subsidence of over a thousand feet, and a subsequent upheaval to a similar altitude." The beds overlie also the other rocks of the country. The shells are largely the same that occur in the Champlain deposits of the St. Lawrence River and the Labrador and Maine coasts: such as, *Saxicava rugosa*, *Mya truncata*, *Cardium Islandicum*, *Tellina calcaria*, *Astate borealis*, *Pecten Grœnlandicus* and others. The paper particularly describes the several localities, and is followed by a note by Mr. Jeffreys, giving a list of the fossils observed and their localities.

10. *A Seal from the Leda Clay (Champlain beds) of the Ottawa valley.*—Dr. Dawson describes this seal as the common Greenland species, *Phoca Grœnlandica.* It occurs in the clays of Green's creek, which have "afforded beautiful specimens of the Capelin and other fishes, and also of marine shells of northern and cold-water types."—*Canadian Naturalist.*

11. *Notes on the Mineralogy and Petrography of Boston and vicinity;* by M. Edwards Wadsworth.—The rocks examined by Mr. Wadsworth are all from the vicinity of Boston. They are proved to belong to the doleritic type, although they have in the past been classed by different authors under various names as "diorytes," "syenites," etc. The microscopical examination has shown that the normal and essential constituents of the rocks are augite, feldspar, and magnetite; they contain also apatite, olivine, pyrite, hornblende, and perhaps biotite. The rocks from most of the localities are marked by a greater or less degree of alteration, in some the change being slight, while in others the decomposition-product "viridite" forms with magnetite the mass of the rock. The rocks which have previously been called "diorite" and "trap" are considered to be identical and of the same age, while the "greenstones" which show more alteration are regarded as older. The localities for the different rocks examined, are stated, and the microscopical characters given with care and minuteness.

Mr. Wadsworth gives in the beginning of his paper a list, covering six pages, of the published articles upon subjects connected with the mineralogy and geology of Eastern Massachusetts.—*Proc. Boston Nat. Hist. Soc.*, xix, 217, May, 1877.

12. *Analysis of Samarskite from Mitchell Co., North Carolina;* by Prof. C. F. Rammelsberg.—The North Carolina samarskite has already been analyzed by Miss E. H. Swallow, Prof. O. D. Allen and Dr. J. Lawrence Smith (see this Journal, III, xiii, 362, and xiv, 130). Prof. Rammelsberg, who has contributed so much to our knowledge of this group of minerals, has also published an analysis of the same mineral. It is as follows:—

Cb_2O_5	Ta_2O_5	SnO_2	UO_3	$Fe_2O_3(Mn_2O_3)$	Ce_2O_3*	Y_2O_3	ErO	SiO_2
41·07	14·36	0·16	10·90	14·61	2·37	6·10	10·80	0·56

* With a little Di. =100·93

The formula deduced from the above is $8\dot{R}_2Nb_6O_{21}+\dot{R}_2U_5O_{21}$ where $\dot{R}=Y_2, Fe_2, Ce_2(Er_2)$, each double atom having an equivalence of six ($Y=92$, $Ce=138$). The American samarskite differs from the Uralian mineral in the high percentage of tantalic acid, and of the element erbium.—*Ann. Phys. u. Chem.*, II, ii, 663.

III. BOTANY AND ZOOLOGY.

1. *Supplementary Note to the Review of Darwin's "Forms of Flowers."* (In January No, pp. 67–71.)—A contributor to the Bulletin of the Torrey Botanical Club, having advanced the idea that the blossoms of *Gentiana Andrewsii* were cleistogamous, because generally seen with corolla closed, we mentioned: 1, that the corolla opened in bright sunshine for a short time, also that humble bees (as others had also recorded) bodily entered even the closed flowers, and would therefore cross-fertilize them: 2, that there was a neat adaptation for ulterior self-fertilization; the pollen long remaining fresh on the ring of extrorse anthers, in such position that when the stigmas of the flower tardily matured, diverged, and became revolute, a part of the stigmatic surface commonly came into contact with the abundant pollen; but this only some time after exposure to the chance of a pollenized entering bee. In the Torrey Bulletin for December last, (vi, 189), Mr. Meehan follows this up with some observations and with other statements which, on account mainly of the singular deductions, may call for a brief remark. He states that *Gentiana Andrewsii* in his neighborhood behaves differently, and that the flowers "do not last a long while." Between this and "a rather long while," the discrepancy is not very explicit, and it is more than done away with by the statement following, that "the ovarium, however, continues to grow, and soon pushes itself through the mouth of the corolla, exposing the stigmatic surfaces which remain in a receptive condition for some time after exposure." This is equivalent to saying that anthesis lasts for a week or two, which is certainly a long while, and doubtless too long. For we are confident that when the ovary, or rather the maturing capsule, is thus exserted out of the mouth of the fading corolla, the stigma no longer "remains in a receptive condition." If it has not been cross-fertilized before this, its day is long passed.

Then follows this: "The only difficulty with me is, that I do not see where the pollen to cross-fertilize is to come from. Mr. Darwin teaches that pollen from the same plant, or from plants growing under similar conditions, is practically no cross-fertilization." This is equivalent to saying that there is no "practical" (meaning useful) cross-fertilization if the plants grow near enough for a bee to fly from the one to the other; which is making what "Mr. Darwin teaches" extinguish cross-fertilization effectually!

Then, "But with me, bees or other insects do not go into one flower on one plant, and then away to another many yards away, then returning, and again going back, continuously going and coming, as a zealous cross-fertilizer, so beautifully arranged by nature, should do, . . . whatever they may do elsewhere." Certainly only the bees in the writer's bonnet behave in this way, or were ever thought to do so.

The article continues thus: "However, it is well to recognize the fact, that plants, and no doubt insects, behave differently in different places. For instance, Mr. Darwin, from English experiments, utterly denies that *Linum perenne* can fertilize itself with its own pollen. He says we may as well "sprinkle over it so much inorganic dust. But a single plant which I brought with me from Colorado, in 1873, bears fruit freely in my garden every year. It shows that how a plant may behave in one place, is no rule as to how it will elsewhere." This extremely remarkable induction of a general rule,—that plants and insects cannot be depended upon for behavior,—is inferred from two instances, one of which has been sufficiently examined; and now a few words may dispose of the other. Mr. Meehan must have noticed (in Forms of Flowers, p. 92) that Darwin's result has been completely confirmed by Hildebrand; and he might have read, on p. 100, the statement, taken from Alefeld, that no American species is heterostyled; and on p. 100, that the Colorado plant, *Linum Lewisii*, of Pursh, the American representative of *L. perenne*, is suspected to be a distinct species, of a sort fully capable of self-fertilizing. This is what Mr. Meehan's observation goes to prove; and so, instead of showing that the behavior of species cannot be relied on, he has unwittingly brought evidence of the correctness of Mr. Darwin's surmise. We looked upon Mr. Meehan's little article as a piece of pleasantry, and should not have referred to it if it had not been noticed abroad as something serious. A. G.

2. *Historia Filicum; an Exposition of the Nature, Number and Organography of Ferns, &c.* By JOHN SMITH, A.L.S., Ex-Curator of the Royal Gardens, Kew, etc. London: Macmillan & Co., 1875; re-issued 1877. 12mo, pp. 429. And with 30 lithographic plates.—The title page proceeds to state, that this volume contains a review of the principles upon which genera are founded, and the systems of classification of the principal authors; with a new general arrangement; characters of the genera; remarks on their relationship to one another, their species, reference to authors, geographical distribution, &c. The plates, drawn on stone by Fitch, illustrate the tribes and leading genera. Mr. Smith is, perhaps, the oldest living pteridologist, and while he had his eye-sight was one of the best. No one else was so intimately and extensively acquainted with Ferns in a living state. In him unusual practical knowledge was combined with no mean talent for systematic arrangement. He was, next to Presl, the first to use the characters of venation, which Brown had cautiously suggested for the definition of genera, and he may be said

to use them with better judgment than Presl, upon an ampler store of materials. His papers, published in 1841–1843, were thought very valuable; and he has since endeavored to make his arrangement more natural. Having nearly a thousand species of Ferns growing under his care, he was led to study their mode of growth, especially of the caudex, and the disposition of the fronds upon it. He finds three distinct modes or types, which we cannot in this brief notice explain, upon which he founds main divisions in classification. One of them is characteristic of an already well-marked primary group, the *Marattiaceæ;* the other two designated as *Eremobrya* and *Desmobrya*, rather neatly, but not so absolutely, divide the typical true Ferns into two great divisions. Succeeding systematists may not assign to these characters so high a place, but they cannot overlook them.

The present handy volume does not take the place of a *Species Filicum.* It merely gives the names of the leading species, "the chief aim of the work being the definition of genera, and their classification, founded on the different modes of growth, venation, and fructification." It has good indexes, a list of Fern authors, the etymology of generic names, and is altogether a useful book both to botanists and amateurs. To those who do not possess a considerable botanical library, it would appear to be indispensable. A. G.

3. *Ferns of North America.* By Prof. DANIEL C. EATON, Part 2.—This prompt continuation argues well for the enterprise, and plates and letter-press are every way commendable. The latter extends to p. 43. The three plates illustrate, *Asplenium ebeneum*, the ambiguous *A. ebenoides*, *Botrychium Lunaria*, *lanceolatum*, and *boreale*, *Cheilanthes lanuginosa* and *C. California*, the letter-press of the latter not included. Referring to the remark at the close of the account of *Asplenium ebeneum*, we suggest that the best of all reasons for adhering to this specific name, is that it is the earliest given for this *Asplenium* as such. A. G.

4. *List of Fungi found in the vicinity of Boston*, part 2, and *Remarks, &c.* From *Bulletin of the Bussey Institution*, vol. ii, pp. 224–252.—The principal bulk and chief interest of this article will be found in what are called the "Remarks," which are critical studies of development, synonymy, &c., of various interesting minute Fungi, beginning with the *Chytridineæ*, unicellular parasites, inhabiting the cells of both aquatic and land plants. By a study of their development, Prof. Farlow shows that a common parasite on *Amphicarpæa*, which has passed for an *Uredo*, and figured under various names, is a *Synchitrium*, a chytridineous genus, and seemingly not distinct from *S. fulgens*, found on *Œnothera biennis.* The only new species described is of this genus; but, what is better for science, a goodly number of species are reduced and properly referred; for instance, *Exobasidium Vaccinii* is made to include *E. Azaleæ* and *E. Andromedæ* of Peck, and *E. discoideum* of Ellis. The synonymy of various species of *Uromyces* is given in detail, and various newly proposed species are referred to Schweinitzian originals. A. G.

5. *Journal of the Linnean Society.*—The last two numbers, containing together almost 200 pages, are filled with the *Spicilegium Floræ Maroccanæ*, by John Ball, based on the collections made in Marocco by Sir Joseph Hooker and himself; a critical study, illustrated by plates of some new or rare species. Thus far it reaches the beginning of the *Umbelliferæ*. A. G.

6. *Guide du Botaniste en Belgique* (*Plantes vivantes et fossiles*), par FRANÇOIS CRÉPIN. Bruxelles and Paris, 1878. pp. 495. 18mo.—A handbook for the tyro in Botany, giving him just the information he most needs, and which, without such help at the outset, he will be a long while in obtaining;—details for herborization; preparation of specimens; formation of herbarium; arrangement for exchanges; nature and characteristics of different sorts of botanical books, and how to use them; how to write them, moreover; and a useful section on the correction of proofs; the botanist's library and what it must needs contain; selection of some of the principal floras and botanical periodicals of the day; a choice of European *Exsiccatæ*; sketch of the principles and methods of vegetable paleontology, with detailed instructions for collecting and managing specimens; systematic lists of the more important works on fossil plants, etc. This all belongs to the first part of the volume, and is general. The second part gives the history of Botany in Belgium, beginning with Jean de Saint-Arnaud of Tournai (born in the year 1200), and coming down to the present day; an account of botanical instruction in Belgium; of the Botanic Gardens, Scientific Societies and Libraries, Herbaria, etc.; then the geographical botany of the country; a catalogue of its fossil flora; full instructions for herborizations in different parts of Belgium, with enumeration of the choice plants of each district; the same for the fossil flora; and, finally, a catalogue of all the works of Belgian botanists, or those who have sojourned in Belgium long enough to be so accounted, from Anselme de Boodt, 1640, and Clusius, downward. No other country has such a hand-book. A. G.

7. *A curious adaptation to insect-fertilization in Trichostema* is described in a letter from Mr. I. J. ISAMAN, of Bangor, California, in a letter to Prof. Bessey of Iowa Agricultural College. The species is *T. lanceolatum*, a very common California annual. Mr. Isaman sends drawings of the two states of the blossom to illustrate his account of the process, which he describes as follows:

"The tube of the corolla is bent upon itself when in its normal condition. On inserting a pin or a small splint, the tube is straightened, and the stamens and pistil are thrown forward, and strike very forcibly upon the back of any intruding insect. I have watched bees for hours, gathering honey from these plants, and have been very much amused by the performance."

The sketches well show how the operation may take place. But we must ask our Californian botanists to verify it, and to ascertain the character and cause of the movement or change ion; also to send us seeds for raising the plant; for this is not in cultivation. Our two Eastern species should be examined in this regard. A. G.

tanical Necrology of 1877.—This contains an unusual of noted names. The following are the principal.

Maria Emma Gray, widow of the late Dr. J. E. Gray, the age of 90, a little before the last year began, viz: 1876, but her name was not in our last necrology. She keen student of *Algæ*, in which order her name is com- ted by the genus *Grayemma*, established by her husband.

ance Smith, widow (since 1828) of Sir James Edward although not herself a botanist, should have a place of the list. She died Feb. 3, 1877, at the age of 104 years! obituary notice appeared in this Journal, April last.

ippe de Notaris, long Professor of Botany at Genoa, ed to Rome in 1872, a cryptogamic botanist of much y, died January 22, 1877, in his 71st year.

elm Hofmeister, Professor at Tübingen, a most distin- vegetable anatomist, died, January 12, 1877, in the 53d his age. His death was noticed in last year's necrology.

ander Braun, Professor of Botany at Berlin University, he best botanists of the age, died, March 29, 1877, in his r. An obituary notice was duly given in this Journal, in e number.

urgeau, one of the best and most persevering botanical rs of our day, who crossed the American continent at the ith Palisser, and made his last collection in Mexico dur- French occupation, died in France early in the year.

istocle Lestibudois, formerly Professor of botany at here he succeeded his father and grandfather, and who author of considerable note in his day, died last summer ge of 80.

Algernon Weddell, English in birth, French by adop- thor of the unfinished *Chloris Andina*, and of monographs *rticeæ*, *Podostemaceæ*, and *Cinchona*, a first-class systematic , died in August last, of heart-disease, at the age of 58.

po Parlatore, Professor and Curator of the Museum at e, author of a *Flora Italiana*, of a monograph of the of Cotton, of the *Coniferæ* in DeCandolle's Prodromus, er works in systematic botany, died at Florence, Septem- t the age of 61.

John Darby, author of a Botany of the Southern States, died in August last, at the age of 73. An obituary ppeared in the December number of this Journal. A. G.

Desmidieæ et Œdogonieæ; by O. Nordstedt and V. ck. II. *Bohusländs Œdogonieer.* III. *Nonnullæ Algæ lcis Brasilienses;* by O. Nordstedt.—The three papers ed appeared in the Proceedings of the Academy of Sci- Stockholm, and each is accompanied by a plate. The described in the first named paper were collected by

Nordstedt himself in Italy and the Tyrol. The desmids were determined by Nordstedt and the Œdogoniæ by Wittrock. The species from Brazil, which with two exceptions are desmids, were collected by A. Glaziou and E. Warming. W. G. F.

10. *A new Species of Chimæra from American Waters*; by T. GILL.—One of the most unexpected discoveries recently made in American ichthyology is that of a species of the genus *Chimæra*, of which a specimen has lately been sent to the Smithsonian Institution. It was caught southeast of the La Have bank, in lat. 42° 40′ N., lon. 63° 28′ W., at a depth of 350 fathoms, with a bait of halibut. A close comparison of the specimen with individuals of the European *Chimæra monstrosa* renders it evident that it does not belong to that species, but is an entirely distinct specific form. It may be named *Chimæra plumbea*, and diagnosed as follows:—A *Chimæra* with the snout acutely produced; the ante-orbital flexure of the suborbital line extending little above the level of the inferior margin of the orbit; the dorsals close together; the dorsal spine with its anterior surface rounded; the ventrals triangular and pointed; the pectorals extending to the outer axil of the ventrals; and the color uniformly plumbeous. By these characters the species is readily separable from the *Chimæra monstrosa* and other species of the genus.—*Proc. Phil. Soc.*, Dec. 22, 1877, Washington.

11. *Dr. W. M. Gabb on Dr. Warring's paper on the growth-rings of exogenous plants a proof of alternating seasons*, in the last volume of this Journal, page 394. (From a letter to the Editors, dated Santo Domingo, Dec. 30, 1877.)—Dr. Warring's paper, in the November number of the Journal, raises a question that I, in my ignorance, thought had been settled long ago. I have lived so long in the tropics that I feel almost as if I had a right to speak " as one having authority." I have had abundant means of observation, and am thoroughly convinced that the conclusions arrived at by the Doctor are correct. I can only demur at one: I have never seen an exogen without rings (3).

The mahogany tree grows here at the average rate of one inch diameter per annum. Now the one-half inch radius certainly contains an average of three rings—which cannot in any manner correspond to wet and dry seasons, to variations of temperature, nor yet to the time of the fructification of the plant.

I might go farther: Santo Domingo is eighteen to nineteen degrees north of the Equator. But I have seen the same facts amply proven ten degrees farther south, where there is practically no variation of temperature, where the rainy season lasts twelve months in the year, and where the trees are, to all appearances, equally vigorous at all seasons of the year.

IV. ASTRONOMY.

1. *On the age of the Sun in relation to Evolution*; by JAMES CROLL.—One of the most formidable objections to the theory of evolution is the enormous length of time which it demands. On

this point Professor Hæckel, one of the highest authorities on the subject, in his "History of Creation," has the following:—"Darwin's theory, as well that of Lyell, renders the assumption of immense periods absolutely necessary. . . . If the theory of development be true at all there must certainly have elapsed immense periods, utterly inconceivable to us, during which the gradual historical development of the animal and vegetable proceeded by the slow transformation of species. . . . the periods during which species originated by gradual transmutation, must not be calculated by single centuries, but by hundreds and by millions of centuries. Every process of development is the more intelligible the longer it is assumed to last."

There are few evolutionists, I presume, who will dispute the accuracy of these statements; but the question arises, does physical science permit the assumption of such enormous periods? We shall now consider the way in which Professor Hæckel endeavors to answer this question and to meet the objections urged against the enormous lapse of time assumed for evolution.

"I beg leave to remark," he says, "that we have not a single rational ground for conceiving the time requisite to be limited in any way. . . . It is absolutely impossible to see what can in any way limit us in assuming long periods of time. . . . From a strictly philosophical point of view it makes no difference whether we hypothetically assume for this process ten millions or ten thousand millions of years. . . . In the same way as the distances between the different planetary systems are not calculated by miles but by Sirius-distances, each of which comprises millions of miles, so the organic history of the earth must not be calculated by thousands of years, but by palæontological or geological periods, each of which comprises many thousands of years, and perhaps millions or milliards of thousands of years."

Statements more utterly opposed to the present state of modern science on this subject could hardly well be made. Not only have physicists fixed a limit to the extent of time available to the evolutionist, but they have fixed it within very narrow boundaries.

Every one will admit that the organic history of our globe must have been limited by the age of the sun's heat. The extent of time that the evolutionist is allowed to assume depends, therefore, on the answer to the question, what is the age of the sun's heat? And this again depends on the ulterior question, from what source has he derived his energy? The sun is losing heat at the enormous rate of 7,000 horse-power on every square foot of surface. And were it composed of coal its combustion would not maintain the present rate of radiation for 5,000 years. Combustion, therefore, cannot be the origin of the heat.

Gravitation is now almost universally appealed to as the only conceivable source from which the sun could have obtained its energy. The contraction theory advocated by Helmholtz is the one generally accepted, but the total amount of work performed by gravitation in the condensation of the sun from a nebulous mass to its present size could only have afforded twenty million years' heat at the present rate of radiation. On the assumption that the sun's density increases towards the center, a few additional million years' heat might be obtained. But on every conceivable supposition gravitation could not have afforded more than twenty or thirty million years' heat.

Professor Haeckel may make any assumption he chooses about the age of the sun, but he must not do so in regard to the age of the sun's heat. One who believes it *inconceivable* that matter can either be created or annihilated may be allowed to maintain that the sun existed from all eternity, but he cannot be permitted to assume that our luminary has been losing heat from all eternity.

If 20,000,000 or 30,000,000 years do not suffice for the evolution theory, then either that or the gravitation theory of the origin of the sun's heat will have to be abandoned.

In a former paper (Quarterly Journal of Science for July, 1877) I have proved from geological evidence that the antiquity of our habitable globe must be at least three times greater than it could possibly be had the sun derived its heat simply from the condensation of its mass. This proves that the gravitation theory of the origin of the sun's heat is as irreconcilable with geological facts as it is, according to Haeckel, with those of evolution, and that there must have been some other source, in addition, at least, to gravity, from which the sun derived its store of energy.

That other source is not so inconceivable as has been assumed, for it is quite conceivable that the nebulous mass from which the sun was formed by condensation might have been possessed of an original store of heat previous to condensation. And this excessive temperature may be the reason why the mass existed in a nebulous or rarefied condition. Now if the mass were originally in a heated condition then in condensing it would have to part not merely with the heat of condensation, but also with the heat it originally possessed.

The question then arises—By what means could the nebulous mass have become incandescent? From what source could the heat have been obtained? The dynamical theory of heat affords, as was shown several years ago (Phil. Mag. for May, 1868), an easy answer to this question. The answer is that the energy in the form of heat possessed by the mass may have been derived from *motion in space*. Two bodies, each one-half the mass of the sun, moving directly towards each other with a velocity of 476 miles per second, would, by their concussion, generate in a *single moment* 50,000,000 years' heat. For two bodies of that mass, moving with a velocity of 476 miles per second, would possess $4,149 \times 10^{38}$ foot-pounds of kinetic energy, and this, converted into heat by the stoppage of their motion, would give out an amount of heat which would cover the present rate of the sun's radiation for a period of 50,000,000 years.

There is nothing very extraordinary in the velocity which we have found would be required to generate the 50,000,000 years' heat in the case of the two supposed bodies. A comet having an orbit extending to the path of the planet Neptune, approaching so near the sun as to almost graze his surface in passing, would have a velocity of about 390 miles per second, which is within eighty-six miles of that required.

It must be borne in mind, however, that the 476 miles per second is the velocity at the moment of collision. But more than one-half of this velocity, or 274 miles per second, would be derived from their mutual attraction as they approached each other. We have consequently to assume an original or projected velocity of only 202 miles per second. If the original velocity was 678 per second, this, with the 274 derived from gravity, would generate an amount of heat which would suffice for 200,000,000 years. And if we assume the original velocity to have been 1,700 miles per second, an amount of heat would be generated in a single moment which would suffice for no less than 800,000,000 years.

It will be asked, where did the two bodies get their motion? It may as well, however, be asked, where did they get their existence? It is just as easy to conceive that they always existed in motion as that they always existed at rest. In fact, this is the only way in which energy could remain in a body without dissipation into space. Under other forms a certain amount of it is constantly being transformed into heat which never can be retransformed back again, but is dissipated into space as radiant heat. But a body moving in void stellar space will retain its energy in the form of motion undiminished and untransformed for ever, unless a collision takes place.

The theory that the sun's heat was originally derived from motion in space is, therefore, for this reason, also more in harmony with evolution than the gravitation theory, because it explains how the enormous amount of energy which is being dissipated into stellar space may have existed in the matter composing the sun untransformed during bygone ages. Or in fact for as far back as the matter itself existed.

In conclusion there are only two sources conceivable from which the sun could have derived his heat. The one is *gravitation*, the other *motion in space*. The former could have afforded only about 20,000,000 or 30,000,000 years' heat, but there is in reality no absolute limit to the amount which may have been derived from the latter source, for the amount generated would depend on the velocity of motion. And when we take into consideration the magnitude of the stellar universe, the difference between a motion of 202 miles per second, and one of 1,700 miles to a great extent disappears, and the one velocity becomes about as probable as the other.

It may be urged as an objection to the theory that we have no experience of bodies moving in space with such enormous velocities as the above. This objection, for the following reason, is of no weight.

No body moving with a velocity exceeding 400 miles per second could remain a member of our solar system; and beyond our system there is nothing visible but the stars and nebulæ. These stars, however, are suns like our own, and visible because, like the sun, they have lost their motion—the lost motion being the origin of their light and heat. Bodies moving in stellar space with these enormous velocities can have neither light nor heat, and, of course, must be invisible to us. They must first lose their motion before the kinetic energy in the form of motion can be transformed into light and heat, so as to constitute visible suns.—*Nature*, Jan. 10, 1878.

2. *Photographs at the Cordoba Observatory.*—Professor B. A. GOULD, Director of the Observatory, in an Address delivered on the occasion of receiving from the Governor of the Province of Cordoba the premiums awarded at the Philadelphia Centennial Exhibition to the Observatory for Lunar and Stellar Photographs, makes the following statements respecting the photographic work of the Observatory. "All the photographs have been made upon plates of glass twelve centimeters long by nine broad, the images of the moon being 3·52 centimeters in diameter before enlargement. The photographs of the moon at the full, and in the last quarter, may, I think, be favorably compared with any I have seen of these phases; and the enlargements, forty-eight centimeters in diameter, being printed in carbon, can never fade. Mr. Rutherfurd's photograph of the first quarter is more sharply defined than ours, and will probably long continue to claim the palm. * * *

"Much of the credit of the stellar photographs is due to the pure air of Cordoba, which is incredibly transparent on those not very numerous occasions when the sky is truly clear. The impressions on glass which I exhibited were of six different clusters; the plate of the cluster *X Carinae*, containing two images each of 185 stars, and that of *Eta Argus*, or *Eta Carinae*, as we now call it, containing 180. I think there can be no reasonable doubt that many of the stars there depicted are quite as faint as the ninth magnitude; and I have the satisfaction of adding that upon other photographs, obtained since then, there are images of stars much fainter still, but how much, I am as yet unprepared to state. Since it is important that all the images should be perfectly round, without the slightest perceptible elongation, and since each of the two exposures should sometimes be for as much as ten minutes, during which period the motion of the telescope must not deviate appreciably from that of the star in its diurnal motion, the prepared collodion thus requiring its full sensitiveness for something like twenty-two minutes in the excessively dry atmosphere of Cordoba, it will be seen that the problem has not been without its difficulties.

"We have already secured measurable photographic impressions of not less than eighty-four celestial objects, of which nineteen are double stars, and the remainder clusters; so that, even should the undertaking remain paralyzed in its present state, the results may be regarded as richly worth the labor, money, and perhaps even the other sacrifices, that they have cost. I may add that the planets *Jupiter*, *Mars* and *Saturn* have likewise been photographed with sufficient distinctness to show clearly the details of light and color on the surfaces of the two former, and the existence of the ring in the latter; but these images have not been sufficiently sharp to permit of successful photographic enlargement."

3. *Moon's Zodiacal Light;* note by E. S. HOLDEN. From a letter to the Editors, dated Naval Observatory, Washington, D. C., February 4, 1878.—In your Journal for February, 1878, p. 88, is a note by M. Trouvelot, on "the Moon's Zodiacal Light," in which he describes a conical luminous appendage about 4½° long, extended on both sides of the moon, which was seen by him April 3, 1874.

In this connection an observation of a similar phenomenon by Messier, in the Mémoires de l'Académie Royale des Sciences, 1771, p. 434, is noteworthy. In this memoir, Messier gives a rough wood-cut of its appearance, from which its length on each side of the moon is shown to be about 2½°.

The condition of the sky, as described by Trouvelot and Messier, appears to have been the same.

In the Comptes Rendus, July 2, 1877, p. 44, M. Hugo describes a similar phenomenon which he saw above the lunar disc, about 4° in length, and in this case, also, the sky appears to have been similarly affected. These are the only cases known to me.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Address of the President of the Royal Society*, at the Anniversary Meeting, November 30, 1877.—The first part of the address relates to the affairs and operations of the society during the past year. After an account of the present constitution and condition of the Meterological Council, and a reference to an anticipated re-constitution of "a National Meterological Office under the undivided control of a man of high scientific attainments," also with some remarks on the report of the Naturalists of the Venus Transit expedition to Rodriguez and Kerguelen Island,—"The President passed on to a review of the scientific results of the Polar expedition, which he said in his judgment, especially the biological results, appeared to have quite come up to our expectations. Considering that but one season was available for collecting and observing (and we all know how short that is in the arctic regions), the results are indeed most creditable to the gentlemen who contributed them. Geology has proved by far the most prolific field of research. Perhaps Botany comes next. The researches in this department and the insects, which have been worked up by Mr. M'Lachlan, prove that between 80° and 83° N., in Grinnell's Land, the conditions for the existence of these organisms are far more favorable than are those of lands a long way to the southward. The flora of the series of channels between 80° and 83° N., the shores of which have been botanized by the officers of the Polar Expedition, has yielded upward of seventy flowering plants and ferns, which is a much greater number than has been obtained from a similar area among the polar islands to the southwestward, and is unexpectedly large. All are from a

much higher latitude than has elsewhere been explored botanically, except the islets off the extreme north of Spitzbergen. The species are, with two single exceptions, all Greenlandic. Spitzbergen, altogether to the south of these positions, contains under one hundred flowering plants and ferns, though its west coast is washed by the Gulf Stream, and its shores have been diligently explored by many trained collectors. Its north coast has yielded fewer plants, and no less than fifteen of the plants collected by the Expedition have not been found anywhere in Spitzbergen. Contrasted with Melville Island, in lat. 75° N., and Port Kennedy, in 72° N., the contrast is even more striking, these well-hunted spots, both so much farther south, yielding only sixty-seven and fifty-two species respectively. This extension of the Greenland flora to so very high a latitude can only be accounted for by the influence of warm currents of air, or by the air being warmed by oceanic currents during some period of the summer; and we look with great interest to the meteorological observations made during the voyage, which are being discussed by Sir George Nares, who hopes to have them completed in a couple of months. The observations on the temperature of sea-water will, he expects, give new information; and great interest is attached to the study of certain warm gales and warm currents that were experienced in lat. 82° and 83° N. May not these phenomena of vegetation and temperature indicate the existence of large tracts of land clothed with vegetation in the interior of Greenland, far within the mountain ranges of its ice-clad coast, and protected by these from the heavier snow-falls, and hence from the accumulation of glacial ice that surrounds it on all sides? Professor Heer, of Zurich, has examined the fossil plants, the most important of which are those he states to be of Miocene age. There are twenty-five identifiable species, of which all but one have been found also in Spitzbergen. This tracing the Miocene flora so far to the northward was one of the principal scientific objects to be accomplished by the Polar Expedition; and the fact that its character continues to be neither Polar nor Arctic, but temperate, supports the hypothesis that during the era in question a vegetation analogous to that now inhabiting the temperate latitudes entirely capped the North Polar area of the globe. Mr. Etheridge has worked at the very valuable collection of Paleozoic fossils, procured by Captain Fielden, and these, with the Miocene and Post-pliocene fossils, have thrown more light on the former conditions of the circumpolar regions than perhaps all those of previous expeditions." * * *

A reference is made to the Challenger Expedition and its scientific collections. As to the study of the latter, it is remarked that "Sir W. Thomson has, with the approval of the Council and the Government, chosen for his collaborators the ablest living specialists, and this irrespective of their nationality. It is creditable to our country that, with but few exceptions, it has supplied entirely competent and willing workers in most of the departments, while their association with such naturalists as Agassiz and Hæckel

; fail to be gratifying to themselves and assuring to the
." * * *

remainder of the President's address was occupied with
ıce to some of the American institutions he visited during
ent tour, followed by some remarks on the flora. These he
n more of a narrative form than characterized the formal
s of the earlier part of the address. Harvard College Ob-
ory is particularly mentioned, and a gratifying account of
rk under the new Director is given. The very limited time
President's command prevented him from visiting other
can observatories or similar institutions. The same was the
s to the natural history museums generally. In fact his
time was given to field-work in the region west of the Great
. The commendation of the U. S. scientific surveys would
ess have been extended to other examples, except that the
re so highly and justly praised was the only one which the
ent had had the opportunity to become personally acquainted

ıe next subject referred to was the United States scientific
s, of which none, the President said, has effected so much
ence as that directed by Dr. Hayden. Its publications, dis-
d with great liberality, are in every scientific library.
a sketch of the history of the surveys, the President con-
,—The most important scientific results hitherto derived
he labors of Dr. Hayden and his parties are unquestionably
ological: such as the delineation of the boundaries of the
eous and Tertiary seas and lakes that occupied more than
asin of the mountains of Central North America, the
t shores of which have yielded a marvelous accumulation
il vertebrates. Over an area of many hundred thousand
miles in North America there have been found, within the
ery few years, beds of great extent and thickness, of all
rom the Trias onward, containing the well-preserved re-
of so great a multitude of flying, creeping, and walking
, referable to so many orders of plants and animals, and
f such gigantic proportions, that the paleontologists of the
with museums vastly larger than our own, are at a loss
ce to exhibit them. So common indeed are some of these
s, and so beautifully preserved, that numbers of them,
lly insects, plants and fishes, are exposed for sale with con-
ery and fruit at the stalls of the railway stations, from the
ı base of the Rocky Mountains all the way to California,
e eagerly purchased by travelers. An examination of some
e fossils has yielded the important fact that in North
a there is no recognized break between the Cretaceous and
y beds, a vast lignitic series occupying a position that has
een disputed, and the evidence of whose fossils is very con-
. In respect of this bed Dr. Hayden, who has traced it
any hundred miles, observes that the character of its pale-
gical, as well as of its strictly geological contents is such,

that it is not a matter of importance whether the entire group be placed in the lower Tertiary or upper Cretaceous; and it is most probable that the testimony of paleontologists will always be as conflicting as it is at present. Professor Marsh, one of the highest authorities in America, has not found that even fossil animals afford a satisfactory solution of the difficulty. 'Invertebrate remains,' he says, 'throw little light on the question;' and he is obliged to assume that 'the line, if line there be, must be drawn where the dinosaurs and other Mesozoic vertebrates disappear, and are replaced by the mammals, henceforth the dominant type.' In reference to the disputed horizons of the Cretaceous and Eocene beds, he concludes that plants afford unsatisfactory measures of geological time—an opinion which I had long ago expressed. We are also agreed as to a chief cause of this being the comparatively low organization of plants, which are hence less subject to the influences of environing conditions; to which cause might be added, almost as a corollary, the feeble conflict among the members of the vegetable kingdom as compared with the animal, their stationary habits, and that consequent duration of similar, if not identical, forms, through long geological ages, which has always appeared to me to be one of the most signal characteristics of the early condition of the higher plants as compared with the higher animals. Other, and perhaps even more sufficient, reasons for plants being so little satisfactory is that their reproductive organs, those upon which the classification is principally based, are rarely preserved, and seldom in connection with the vegetative organs, which are abundantly preserved; and that, with regard to these, the vegetative organs, their prevalent and best preserved characters, outlines and venation, vary in individual species to a surprising degree, and, being repeated in groups otherwise in no way related, become too often fallacious guides. Another result, foretold in respect of other organisms, but ably worked out by Professor Marsh in respect of the vertebrates, is that all the Tertiary beds of North America—Eocene, Miocene and Pliocene—are of older data than the corresponding ones in Europe. This, though apparently supported by his conclusions that the main migrations of animals was from the American to the Asiatic continent (which he deduces from the American, as compared with the European, life-histories of the Edentata, Marsupialia, Ungulata, Rodentia, Carnivora and even Primates, is a very bold generalization. Without presuming to question the abundance and teachings of the American data, I cannot but think that his conclusion as regards migration is, in the present state of paleontology, premature, especially under our almost absolute ignorance of the vertebrate fossils of the continents of Asia and Africa." * * *

A reference is made to "a few of the magnificent collections of vegetable remains, Cretaceous and others, that have been studied and published by Mr. Leo Lesquereux in various reports of the United States Geological Survey, and in separate works issued under its auspices;" and a series of critical remarks upon vegetable paleontology and its peculiar difficulties and liabilities follow. We have not room for these, nor for the concluding portion of the address, which gives in brief summary the impressions in regard to the flora of the country,—the result of personal observation during a rather rapid but favorably-circumstanced reconnoissance from the Atlantic to the Pacific. We add merely the final paragraph of the address.

"I must not close my notice of some of the labors of our scientific brethren in the United States without expressing my admiration of the spirit and the manner in which the Government and people have coöperated in making known the physical and biological features of their country, and my conviction that the results they have given to the world are, whether for magnitude or importance, greater of their kind than have been accomplished within the same time by any people or Government in the older continents. How great would now be our knowledge of the climate and natural features of India had its trigonometrical or revenue surveys been carried out in the same catholic spirit? And what scientific literature can England and its colonies show to compare with that of the United States surveys?"

These extracts were taken from the report in the Times newspaper, of November 30, in advance of the official copy in the Royal Society's Proceedings.

2. *Observations on Hermetically-sealed Flasks opened on the Alps.* In a letter to Professor HUXLEY, from Professor TYNDALL, dated Alp Lusgen, September 18th, 1877.—Though the question of "Spontaneous Generation" is, I believe, practically set at rest for the scientific world, you may possibly deem the following facts of sufficient interest to be communicated to the Royal Society.

I brought with me this year to the Alps sixty hermetically-sealed flasks, containing infusions of beef, mutton, turnip and cucumber, which had been boiled for five minutes in London and sealed during ebullition. They were packed in sawdust, and when opened at Bel-alp the drawn-out and sealed ends of six of them were found broken off. These six flasks were filled with organisms, while the remaining ones were pellucid and free from life.

Two or three of them were subsequently broken by accident, but for six weeks fifty of the flasks remained perfectly clear.

At the end of this time I took twenty-three of them into a shed containing some fresh hay, and there snipped off their sealed ends with a pair of pliers. The air of the hay-loft entered to fill the vacuum produced by the boiling in London. Twenty-seven other flasks were taken immediately afterward to the edge of a declivity, which might almost be called a precipice, with a fall of about a thousand feet. A gentle breeze was blowing from the mountains, which were partly snow-covered and partly of bare rock, toward the precipice. Taking care to cleanse my pliers in the flame of a spirit-lamp, and to keep my body to leeward of the flasks, I snipped off their sealed ends.

The two groups of flasks were then placed in our own little kitchen, where the temperature varied from about 65° to 90° Fahrenheit.

Result:—Twenty-one of the twenty-three flasks opened on the hay-loft are filled with organisms; two of them remain clear.

All the flasks opened on the edge of the precipice remain as clear as distilled water. Not one of them has given way.

This is a striking confirmation of the experiments of Pasteur upon the Mer de Glace.—*Proc. Roy. Soc.*, No. 184.

3. *Researches on the Effect of Light upon Bacteria and other Organisms;* by Arthur Downes, M.D., and Thos. P. Blunt, M.A. —The authors, after giving the details of their careful and extended observations announce the following conclusions.—The deductions which we draw from these simple experiments may be summed up as follows:—

(1.) Light is inimical to the development of *Bacteria* and the microscopic fungi associated with putrefaction and decay, its action on the latter organisms being apparently less rapid than upon the former.

(2.) Under favorable conditions it wholly prevents that development, but under less favorable it may only retard.

(3.) The preservative quality of light, as might be expected, is most powerful in the direct solar ray, but can be demonstrated to exist in ordinary diffused daylight.

(4.) So far as our investigation has gone it would appear that it is chiefly, but perhaps not entirely, associated with the actinic rays of the spectrum.

(5.) The fitness of a cultivation-liquid to act as a nidus is not impaired by insolation.

(6.) The germs originally present in such a liquid may be wholly destroyed, and a putrescible fluid perfectly preserved by the unaided action of light.

Although there are many vital phenomena, both of plant-life and of animal, whether in health or disease, to the elucidation of which may be applied this quality of light (now demonstrated, so far as we are aware, for the first time), we have endeavored in this paper to confine ourselves to the plain facts of our observations, and have studiously avoided speculation and theory. We cannot, however, refrain from offering one comment on the striking antagonism between these facts and many views that have hitherto prevailed on the relation of light to life. This relation has been principally investigated as regards the chlorophyl-cell; but chlorophyl may be regarded as simply an organ of nutrition adapted to special circumstances, and differing essentially in its vital phenomena from the true cellular tissue of the plant and its protoplasmic contents.

It appears to us that the organisms which have been the subject of our research may be regarded simply as individual "cells" or minute protoplasmic masses specially fitted by their transparency and tenuity for the demonstration of physical and other influences. May we not expect that laws similar to those which here manifest themselves may be in operation throughout the vegetable, and perhaps also the animal, kingdom wherever light has direct access protoplasm? On the one hand we have chlorophyl, owing its ry existence to light, and whose functions are deoxidizing; the other the white protoplasm, or germinal matter, oxidizing its relations, and to which, in some of its forms at least, the lar rays are not only non-essential, but even devitalizing and jurious.

This suggestion we advance provisionally and with diffidence; r do we wish to imply that the relations of light to protoplasmic atter are by any means so simple as might be inferred from the ove broad statement.

To this the following is added by the authors as the inference m other experiments described in a postscript.

This remarkable fact, then, appears to follow as a deduction, at a vacuum (or approximation to such) which of itself is a ndition antagonistic to the development of *Bacteria*, neverthe- s shields these organisms from the germicidal effect of light.*

'oc. *Roy. Soc.*, No. 184.

4. *Expenditures for Universities in Germany.*—Some inter- ting details, on the contributions of the State to the univer- ies, as well as on other points, were given in a recent num- r of the Academy by Professor Ray Lankester:—

"The sum expended by the North German States on the twenty iversities belonging to them is annually more than £500,000. e Imperial Government has expended upon the new University Strassburg alone £70,000 in one year. The University of ipzig alone receives annually from the Saxon Government over 0,000. There are eight universities in North Germany which e little, if at all, less costly, and there are eleven of smaller size ich receive each from £8,000 to £20,000 annually.

"In North Germany there is one university to every two million abitants; in Austria there is one to every five million; in itzerland one for each million; in England one to every seven llions. In the twenty North German universities there are 1,250 ofessors.† In the British Islands we ought to have sixteen iversities and 1,000 professorships in order to come up to the me level in this respect as North Germany. The stipend (apart m fees) of a professor in a German university ranges from 00 to £600 a year. As a rule, at the age of five-and-thirty, a n in this career may (in Germany) count on an assured in- me of £400 a year (with retiring pension). The expenditure attendants, libraries, laboratories and officials may be calcu- ed as being (in a well-conducted university) more than equal amount to the total of the professor's stipends. Taking the *erage* German professorial stipend at only £200 a year, we find at £250,000 must be spent annually on this item alone in the rth German States.

"In order to equip and carry on sixteen universities in this ntry which should bear comparison with the German universities, we require not less than an immediate expenditure of £1,000,000 sterling in building and apparatus, and an annual expenditure of from £500,000 to £800,000."

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* We wish, however, to make it clear that we by no means insist on this ex- nation; the facts, indeed, admit of other explanations.

† *i. e.*, We presume professors strictly so-called, exclusive of "privat-docenten."

When we add to the Government subsidy the income of the universities from other sources, the sum is enormously increased. The half-million, moreover, does not include the occasional grants of the Government for special purposes. Some idea of the magnificence of these was shown in our recent "University Intelligence," where it was stated that in the budget submitted to the present Prussian House of Deputies are the following items:—Erection of the German Industrial Museum, 998,000 mk.; erection of a Polytechnic in Berlin, 8,393,370 mk.; erection of an Ethnological Museum in Berlin, 1,800,000 mk.; and for the Berlin University, erection of an Herbarium, 422,000 mk.; of a Clinic, 1,955,000 mk.; of a new building for a second Chemical Laboratory, as well as of a Technical and Pharmaceutical Institute, 967,000 mk.

Of the nature and extent of the scientific teaching in German universities some idea may be formed from the subjects represented by the teaching staff at Berlin, which may fairly be taken as a type of the whole. In Berlin then we find that there are (excluding the privat-docenten) five professors of mathematics, two of astronomy, seven of chemistry, five of physics, three of geology, four of botany, two of zoology, one of meteorology, two of geography, one of anthropology and one of agriculture—physiology and comparative anatomy being well represented in the medical faculty, and we might well have included among teachers of science those who devote themselves to the scientific investigation of languages. But a mere statement of the number of teachers gives no adequate idea of the means at the command of a German University for training its students in science. The number of teachers in each subject secures that its various departments will be thoroughly worked out, and gives a student a chance of following out any specialty he may take up; this is made still further possible by the number and variety of institutions, museums, laboratories, collections, &c., attached to each university, not to speak of its large and comprehensive library. In connection with Berlin alone there are twenty-three scientific "Anstalten," as they are called, for practical investigation in connection with the various faculties. Had we taken the numerous "Realschulen" and the high and polytechnic schools into account, where an education can be obtained quite equal to that obtainable at most of our universities and colleges, it would have been seen that higher education in Germany leaves little to be desired.—*Nature*, Dec. 6, p. 104.

5. *Earthquake of Nov.* 15, 1877.—In addition to the notice on page 21 of this volume, the United States Weather Review for November contains reports from thirty-five stations, from which it appears that the shock was felt throughout the whole of Iowa and Nebraska, extending also into Kansas and Missouri on the South, and into Dakota and Minnesota on the North. In Omaha

three, and in North Platte, Lincoln and West Point, Neb., two shocks were felt. In North Platte and Columbus, Neb., and in Sioux City, Iowa, walls of buildings were cracked. The times given are still very discordant, varying all the way from 10.40 A. M., to 12.20 P. M., Omaha time. The time at Omaha, 11.45 A. M., is probably not far from correct. The December number of Peterman's Mittheilungen, contains a long article on the Iquique earthquake of May 9, 1877, giving valuable data in regard to shock and ocean wave. C. G. R.

6. *Memoirs of the Geological Survey of India, Palæontologia Indica.* Section II, No. 2, contains Jurassic (Liassic) flora of the Rajmahel Group in the Rajmahel Hills, by OTTOKAR FEISTMANTEL, M.D. Calcutta, 1877. 4to, with 13 plates, including descriptions of several species of *Pterophyllum* and other Cycadeous plants, besides Ferns of the genera *Cyclopteris*, *Pecopteris*, *Alethopteris*, *Asplenites*, *Thinnfeldia*, etc.

7. *Volunteer Weather Service.*—A Volunteer Weather Service has been started in Missouri. It is undertaken under the auspices of Washington University, St. Louis, which will be recognized as the "central station." The circular issued is signed by William S. Eliot, President of the University, and Francis E. Nipher, Professor of Physics.

8. *Beiblätter zu den Annalen der Physik und Chemie;* Publishing House of Johann Ambrosius Barth, Leipzig.—The Annalen der Physik und Chemie, which was edited for upwards of fifty years by Professor Poggendorff, is well known by physicists as occupying the first place among German Physical Journals. The *Beiblätter* form a supplementary though independent series, containing abstracts of all physical articles published in the proceedings of societies, in scientific journals, or in other forms, not only in Germany, but also in all other civilized countries. The volume for the year 1877, lately completed, is thus a complete Repertorium for all the recent advances in physical science. The value of such a summary to workers in this branch of science, to whom much of the literature would be otherwise inaccessible, can hardly be overestimated.

OBITUARY.

M. ANTOINE CÉSAR BECQUEREL died on the 19th of January, at the advanced age of nearly ninety years. He was born at Châtillon-sur-Loing (Loiret) on the 7th of March, 1788. M. Becquerel was educated in the Polytechnic School, which he left as an engineer officer in 1808. He served in Spain and took part in several sieges under the orders of Marshal Suchet. In 1814 M. Becquerel was named Inspector of the Polytechnic School, and he quitted the army in 1815.

M. Becquerel was elected a Member of the Académie des Sciences in April, 1829, and a Corresponding Member of the Royal Society of London in 1837, the Copley medal being awarded to him for his researches in electricity. He was Professor of Physics in the Muséum d'Histoire Naturelle. A large number of memoirs on different branches of electricity, to which M. Becquerel devoted his especial attention, will be found in the Comptes Rendus of the Academy of Sciences. We may more particularly name Mémoire sur les Caractères Optiques des Minéraux (183[illegible]). Sur les Propriétes Électro-Chimiques des Corps Simples et leurs Applications aux Arts (1841), and Mémoires sur la Reproduction Artificielle des Composés Minéraux, à l'aide de Courants Électriques très faibles (1852). His researches on animal heat, and other applications of physics to physiology, on which subjects memoirs will be found in the Comptes Rendus, were of a high class.

M. Becquerel was a voluminous writer on science, the most important of his works being, Traité de l'Électricité et du Magnétisme (1834–1840, in seven vols.), Traité d'Électro-Chimie, and his Traité de Physique appliquée à la Chimie et aux Sciences Naturelles. Beyond these he published, in connection with his son, M. Edmund Becquerel, several works on meteorology, on agricultural chemistry, on the influences of forests on climate, and on the several divisions of electrical science, to which the father and son had devoted the largest portion of their lives.—*Athenæum*, Jan. 26.

M. REGNAULT.—M. Henri Victor Regnault died at Paris, almost simultaneously with M. Becquerel, on the 21st of January.

M. Regnault was born on the 21st of July, 1810, at Aix-la-Chapelle He was a student of the Polytechnic School, and shortly after leaving that school he became Ingénieur en Chef des Mines. In 1840 he became Professor of Physics in the College of France and of Chemistry in the Polytechnic School. In the same year he was elected a Member of the Académie des Sciences, and in 1854 he became Director of the Manufactory of Porcelain at Sèvres. In 1852 M. Regnault was elected a Foreign Member of our Royal Society, and at different times the Copley and the Rumford Medals were presented to him. M. Regnault was also a Corresponding Member of the Academies of Berlin and St. Petersburg. In the Annales de Chimie et de Physique and in the Comptes Rendus des Séances de l'Académie will be found numerous memoirs by this eminent chemist. One of the most important works published by M. Regnault appeared in the twenty-first volume of Mémoires de l'Académie des Sciences, under the title of Relation des Expériences entreprises par Ordre de M. le Ministre des Travaux Publics, et sur la Proposition de la Commission Centrale des Machines à Vapeur.' These researches remain a standard authority upon all questions relating to the theory and practice of the use of steam as a motive power.

M. Regnault was the father of the celebrated painter who fell, fighting for his country, at the siege of Paris.

M. Regnault published a Cours Élémentaire de Chimie, in four volumes, Première Notions de Chimie, and a Traité de Physique. The Cours Élémentaire has been translated into several European languages, and the other works of M. Regnault are highly appreciated in this country as in France.—*Ibid.*

APPENDIX.

Art. XXXIII.—*Notice of New Dinosaurian Reptiles*; by Professor O. C. Marsh.

N addition to the Jurassic reptiles already described by the ter,* several others of interest are now represented in the le Museum. Among these are a number of Dinosaurs of antic size, and others of diminutive proportions. Nearly all from the Atlantosaurus beds of the Rocky Mountains. Most the larger specimens belong to the *Atlantosauridæ*, a group rked by some interesting characters not before observed in iosaurs. The more important of these characters, so far as present known, are, the pneumatic cavities in the vertebræ; sacrum with only three or four vertebræ, and a correspond- short ilium; the large fore limbs; and the presence of five ll developed digits in both the manus and pes. The latter s ungulate, and essentially plantigrade. The carpal and sal bones are not coössified with the long bones, and the rd trochanter of the femur is rudimentary or wanting. The own genera are *Atlantosaurus* (*Titanosaurus*), *Apatosaurus*, l *Morosaurus* described below.

Atlantosaurus immanis, sp. nov.

he present species was vastly larger than any land animal ier recent or fossil, hitherto described. It is indicated by ious well preserved remains, of which the most character- c is the femur. This bone has no true head, and no dis- t third trochanter. The proximal end and upper half of shaft are compressed transversely. The inner condyle of distal end is proportionally large, and on the outer one, fibular ridge is well marked. This femur is *over eight feet* inches, or 2,500^{mm}) in length. The transverse diameter the proximal end is 25 inches (635^{mm}), and the antero- terior diameter of the inner condyle of the distal end 18 inches (475^{mm}). A comparison of this bone with the iur of a Crocodile (*C. Americanus*), would indicate for the sil species, supposing the two reptiles to have the same pro- tions, a length of about one hundred and fifteen feet!

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* This Journal, xiv, pp. 87, 254, 513, 514.

The other bones found near the femur are proportionally gigantic, one caudal vertebra having a transverse diameter of over 16 inches (420·^{mm}). That this reptile when alive was near one hundred feet in length, is probable, although it may have been much less.

The only remains of this monster at present known are in the Yale College Museum. They are from the Upper Jurassic of Colorado.

Morosaurus impar, gen. et sp. nov.

This genus is allied to *Apatosaurus* and *Atlantosaurus*, but may be distinguished from them by the sacrum, as well as by other characters. The former has but three sacral vertebræ, while the present genus has four. The transverse processes are vertical plates, except at their expanded ends, which extend below the inferior surface of the centra. The latter are also more fully ossified than in *Atlantosaurus*. The first sacral vertebra has its articular face somewhat convex, while the articulation of the last sacral vertebra is concave.

The present species is represented by various remains, the sacrum being most characteristic. Its principal dimensions are as follows:

Length of sacrum	535·^{mm}
Transverse diameter of anterior articular face	215·
Transverse diameter of posterior articular face	190·
Expanse of transverse processes of second vertebra	395·

This sacrum indicates a reptile at least twenty-five feet in length. It was found with other remains in the Atlantosaurus beds by Mr. S. W. Williston, of Yale College Museum, to whom science is indebted for many important discoveries in the Rocky Mountain region.

Allosaurus lucaris, sp. nov.

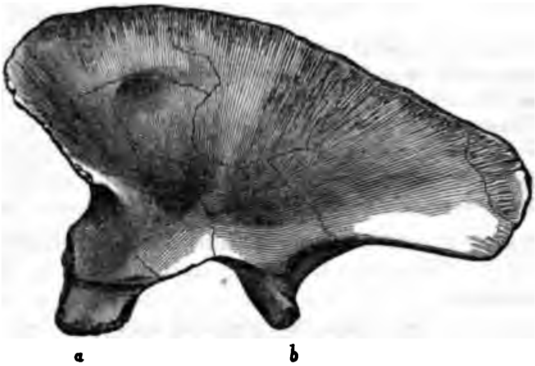
The peculiar genus named by the writer *Allosaurus* proves to be very different from the Dinosaurs found with it, and to represent a distinct family, *Allosauridæ*. A second species, apparently of this genus, is indicated by some characteristic remains among which is an anterior dorsal vertebra. This has the anterior articulation of the centrum somewhat convex, and the posterior face concave. The sides of the centrum are so deeply excavated that only a narrow keel is left below, and there are large cavities in the interior. The length of this centrum is 69·^{mm}; the vertical diameter of the anterior face, 81·^{mm}; and the width of this face, 33·^{mm}. The articulation for the rib is at the anterior border, just below the suture of the neural arch.

This specimen is from the Upper Jurassic of the Rocky Mountains, and belonged to a reptile eighteen or twenty feet in length.

***Creosaurus atrox*, gen. et sp. nov.**

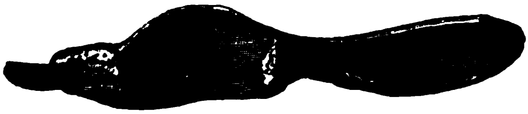
This genus is nearly allied to *Dryptosaurus* (*Lælaps*), and was the carnivorous enemy of the huge *Atlantosauridæ*. It is indicated by various remains in excellent preservation, among them the ilium represented below. The teeth referred to the present species have the crowns more or less trihedral, and the cutting edges crenulated. The metapodial bones preserved are elongated, and the terminal phalanges supported sharp claws. The vertebræ known are biconcave, and the terminal caudals are much elongated.

1.



Left ilium of *Creosaurus atrox* Marsh. Outside view.

2.



Inferior view. Both one-tenth natural size.

The following measurements indicate the size of this reptile:

Antero-posterior diameter of left ilium	700·mm
Vertical diameter	425·
Length of metatarsal	277·
Transverse diameter of proximal end	72·
Transverse diameter of distal end	79·
Length of distal caudal vertebra	78·
Transverse diameter of proximal end	33·
Transverse diameter of distal end	31·

This animal was about twenty feet in length. The remains at present known are from the same horizon as those above described, and were collected by Mr. S. W. Williston.

Laosaurus celer, gen. et sp. nov.

The present genus is indicated by various remains of small Dinosaurs, of two or more species. The long bones are not hollow like those of *Nanosaurus*, but their walls are thick, and the cavities small. The vertebræ preserved are biconcave, and the neural arches loosely united to the centra. The dorsal and anterior caudals are more elongated than in most Dinosaurs. The phalanges are so avian in character, that they would readily be taken for those of birds. The anterior limbs were much smaller than the posterior.

The following are some of the dimensions of the present species:

Length of median caudal vertebra	24·$^{\mathrm{mm}}$
Vertical diameter of anterior articulation	17·
Transverse diameter	16·
Greatest diameter of proximal end of ulna	19·5
Length of proximal phalanx of pes	29·
Length of second phalanx of pes	21·
Length of third phalanx	16.

The remains at present known indicate an animal about as large as a fox. They are from the same horizon as the species described above.

Laosaurus gracilis, sp. nov.

A second species, much smaller than the above, is represented by well preserved remains of various parts of the skeleton. Its size is indicated by the following measurements:

Length of lumbar vertebra	16·$^{\mathrm{mm}}$
Transverse diameter of anterior face	18·
Transverse diameter of posterior face	17·
Length of median caudal vertebra	16·
Transverse diameter anterior face	12·
Greatest diameter of proximal end of ulna	17·

The present species is from the same locality and horizon as the one above described.

This reptile is the smallest known Dinosaur, with the exception of the diminutive species of *Nanosaurus* (*N. agilis* and *N. victor*). The latter genus possesses some very peculiar characters, and represents a distinct family, *Nanosauridæ*.

Yale College, New Haven, February, 1878.

THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[THIRD SERIES.]

ART. XXXIV.—*On the Surface Geology of Southwest Pennsylvania, and adjoining portions of Maryland and West Virginia;* by JOHN J. STEVENSON, Professor of Geology in the University of New York.

THE following article contains a brief summary of the results obtained by me during three years' labor in connection with the Second Geological Survey of Pennsylvania. The detailed statement will appear elsewhere.

The area in which observations were made covers in all more than 10,000 square miles. It embraces that portion of Pennsylvania lying south from the Ohio and Conemaugh Rivers and west from the Alleghanies; includes a large part of West Virginia and Maryland lying on both sides of the Alleghanies of Virginia; and has the channel-ways of four great rivers, the Monongahela, Cheat, Youghiogheny and Potomac, lying partly within it.

Along all the streams there are fine terraces covered with detritus containing many polished fragments, which have been transported from a greater or less distance; these terraces fall down stream but not so rapidly as do the present stream-beds; and the terraces of tributaries are continuous with those of the main streams.

A second series of benches appears throughout this whole region and seems to be characteristic of a much wider area than that in which observations were made. The benches of this series evidently differ in origin from those of the lower series; their detrital coating contains little clay, no transported fragments and consists almost wholly of sand. They are almost absolutely *horizontal;* they do not merge into the lower series, but as they often form divides, the lower series ends up in some member of the upper. These horizontal benches begin at 1,100 feet above mean tide; they line the mountain slopes; they curve round the conical hills, and oftentimes they are indicated solely by the leveled crowns of the higher knobs.

As the whole area lies south from the southern line of northern drift, none of that material occurs. In like manner there is no evidence whatever in any portion of the region or at the headwaters of any of the rivers that glaciers were ever present.

The Horizontal Benches.

Benches belonging to this series have been fully recognized over the whole area examined. The altitudes are as follows:—

Above tide.	Above tide.	Above tide.
1. 2,580 feet.	8. 1,570 feet.	15. 1,290 feet.
2. 2,400 "	9. 1,520 "	16. 1,270 "
3. 2,323 "	10. 1,475 "	17. 1,240 "
4. 2,288 "	11. 1,445 "	18. 1,190 "
5. 2,063 "	12. 1,420 "	19. 1,130 "
6. 1,820 "	13. 1,380 "	20. 1,100 "
7. 1,690 "	14. 1,350 "	

Of these benches, those below No. 11 were recognized at many localities within an area of more than 5,000 square miles; the higher ones were seen at widely separated localities, but their level is reached only on the sides of the mountains, or in elevated valleys like the Ligonier or the Salisbury Basin. The extreme variation in level is barely twenty feet in any case and in most of the benches the altitude is accurately the same at all places. No. 17 shows a variation of eighteen feet, and I am much inclined to believe that I have confounded two benches which are close together. These benches are parallel.

The arrangement of the horizontal benches is finely shown on the National road between Chestnut Ridge and a high hill fifteen miles west from the Monongahela River.

Standing on the highest point crossed by that road between Chestnut Ridge and the Monongahela River, one finds himself standing on an island of No. 14, or 1,350 feet above sea-level. Below him, an almost continuous plain of No. 15 stretches for a long distance north and south and is broken only by gaps which the larger streams make through this small ridge on their way to the Monongahela River. He sees also that this plain is the divide between two valleys, one at the east between this and Chestnut Ridge, and the other at the west, in which the Monongahela flows. The latter is uninterrupted as far as the eye can reach, but the former is crossed by strips of the main plain of Brush Ridge, as well as by lower benches which break its continuity and convert it into a succession of basins.

From the summit-plain of this Brush Ridge, the surface falls off in regular steps.

If, now, the observer turn his attention to the region lying directly west from the Monongahela River, he will see that No. 15 is a broad continuous plain beyond that river, but that still farther back toward the west, the fourteenth bench, on an island of which he is standing, forms a similar plain, while still farther back, No. 13, with an altitude of 1,380 feet above tide, stretches northward and southward and is broken only by the narrow valleys in which the larger streams flow.

Should the observer's position be changed to Hillsborough, fifteen miles west from the Monongahela River, where the elevation is about 1,500 feet above tide, he will see that No. 13 is of great extent north and south, while back of it the country rises to a still higher level, again and again, until it reaches No. 11 at 1,445 feet above tide.

From the river westward to Hillsborough, or rather to a ridge passing nearly north and south at three miles east from that village, the surface rises in a succession of steps which are beautifully marked. From the hill-top at Hillsborough, the descent to the river is very handsomely shown.

That these benches are simply the result of remodeling valleys formed long before the agent making the benches began to work, is shown by the distribution of the benches themselves; for these benches line the sides of long narrow valleys reaching far inland from the rivers, and breaking through ridges bearing higher members of the series. The erosion producing these valleys began, I believe, even before the anticlinal axes had been elevated sufficiently to affect the topography. The main streams of the present drainage system break through all the bold axes west from the Alleghanies of Virginia.

The recent origin of the benches is amply clear from their condition of preservation. If they had been of ancient origin their detrital coat would have been removed, for we cannot doubt that rain fell on these as well as along the lines of the water-courses; in like manner the crowns of the hills would have been rounded, especially where the rock is a soft shale inclined at a considerable angle, as is the case in southern Fayette county.

The whole structure of these benches shows them to be simply old beach lines marking successive stages of emergence from a flood of waters. They are horizontal, parallel, and are covered with detritus derived from the immediate vicinity. They extend over an enormous area both east and west from the Alleghanies of Pennsylvania, which, within that State at least, form the water shed between the Atlantic Ocean and the Gulf of Mexico.

As they are so wide-spread, it is impossible to account for them by the draining away of a great lake or by the action of a great flood sweeping over the whole region. They can be no other than sea-beaches, marking stages in the withdrawal of the ocean. This supposition involves a submergence of the land to a depth of fully 2,600 feet, if we regard the higher benches as due to the same cause with the lower ones, and the submergence would have to be somewhat greater to account for the even crests of the Alleghanies and other ridges of the Appalachian region west from the Blue Ridge.

The River Terraces.

The persistent terraces are five in number and their relations are shown at the junction of Cheat and Monongahela Rivers. Three or four miles north from the West Virginia line, they are as follows:—

	Above river.
1.	280 feet.
2.	210 "
3.	180 "
4.	80 feet.
5.	20 "

The absolute elevation of the highest terrace at this locality is 1,050 feet above tide.

These fall down stream and are covered by detritus, consisting of irregularly bedded sand, clay or gravel, in which are transported fragments which have been rounded by the action of running water. When followed up any of the smaller, or for long distances on the larger, streams, these terraces are seen to differ in degree of slope, so that each lower one is merged into the next higher until that, which at the mouth of the stream is the river " bottom," becomes the only terrace and at last is lost in the lower horizontal benches where the stream takes its rise.

The terraces occur at the same elevation on both sides of streams, being divided by the channel-way just as the present " bottom " is divided. In some instances a terrace is wanting on one side; but there it is clear enough that that corrasion was confined to one side, for the terrace is unusually wide on the other. The same condition is often seen in the flood plain of the river now.

As stated in my report for 1875, these terraces are simply shelves in the rock on which rests a thin coat of detritus. Mr. G. K. Gilbert, in his memoir on the geology of the Henry Mountains (not yet published), describes similar terraces as occurring there, though it does not appear that they are found at the same height on both sides of the streams.

The terraces on the Ohio, below Pittsburg, consist largely of northern drift brought down by the Allegheny and Beaver Rivers, so that they certainly date from a time later than that at which the drift was spread over northern Pennsylvania. Along the Monongahela and other rivers south from the Ohio, no such material occurs, but the deposits afford sufficient evidence of another kind to enable us to fix their origin within comparatively recent times. At New Geneva on the Monongahela near the West Virginia line, the highest terrace has a thick coating, in which a layer known as the "swamp clay" holds much half rotted wood, such as is frequently seen in peat bogs. In the same neighborhood the third terrace shows many *Unio* shells in an advanced stage of decay. A similar condition exists on the same terrace at Morgantown, farther up the river in West Virginia. At Belvernon, on the same river, near the northern line of Fayette county, this terrace yields many fragments of wood. In this way it can be shown that the deposits on the first, third, fourth and fifth benches are of recent origin.

Since these deposits are of recent origin, there would seem to be good reason for supposing that the valleys through which their streams flow are also of recent origin, at least so much of them as lies below the level of the highest terrace. But it has been suggested that these terraces are only the result of reworking the sides of the valleys, which had been eroded previously.

The structure of the valleys below the highest terrace is very different from that above that terrace; for in the upper portion the sides are gently sloping, whereas below it they become steep at once. Above the line of that terrace, the smaller valleys are broad swales, while below it the streams flow in gorges. The abruptness of this change from gentle to steep walls shows that the lower portion of the gorges was eroded after the upper portion had acquired approximately its present form. There is not the slightest atom of evidence to lead any one to suppose that the valley of the Monongahela was ever filled with gravel, and the stream now flows on a rocky bed from its source to within a very few miles of its mouth.

These river terraces are relics of river beds, which at one time stretched across the valleys, just as the river "bottoms" now do; and the valleys below the line of the highest terrace have been eroded since the drainage system was reëstablished by withdrawal of the ocean below the lines of the former stream beds.

Conclusions.

The general conclusions to which I have come are:—1. That the erosion, to which is due the general configuration of the surface above the highest river terrace, began even before the elevation of the anticlinal axes and continued until the whole region was submerged in post-glacial time. 2. That the horizontal benches are due to re-working of preëxisting valleys, and that they mark stages of rest during emergence of the continent from the ocean which covered it. 3. That the river terraces and the valleys which they line, were formed after the drainage system had been reëstablished by withdrawal of the water to a level below that at which the streams had previously flowed.

It will be seen at once that this last conclusion leads to one of wider application.

So long a time elapsed between the beginning of the drainage and the coming of the great flood, that deepening of the water-ways kept pace only with the general wasting of the adjacent country, for we find comparatively gentle slopes down to the line of the highest terrace. But after the drainage had been reëstablished, the rate of flow must have been more rapid than previously, so as to increase the corrasive power of the streams to far beyond what it had been, for in the newer portion of the valleys the sides are abrupt. There must then have been a change of altitude in respect to tide-level, to lead to this increased rate of flow and the consequent increased speed with which the channel ways were deepened.

It would appear then that, after the submergence following the Glacial period, the continent rose to a greater height than it had before the submergence, or that the ocean was drawn off to a lower level than before; the result in either case being the same—to depress the mouths of the great rivers, to increase the fall of the streams and therefore to cause the rapid deepening of the water-ways.

ART. XXXV.—*On the Driftless Interior of North America*; by JAMES D. DANA.

1. *Driftless area of Central and West-Central North America.*

IN the number of this Journal for April, 1875,* I have accounted for the absence of the northern drift from the interior of North America—over the great region between Western Iowa and the Sierra Nevada in California and the country north to an undetermined distance—on the ground of the dryness of the climate in connection with the heat of the summer; and I referred for the facts on the former of these points to Mr. Charles A. Schott's very valuable memoir on the "Precipitation in rain and snow in the United States," published in 1872 by the Smithsonian Institution. As the subject is one of great interest I here reproduce portions of two of Mr. Schott's charts (see beyond); one (No. 1) giving the lines of equal precipitation for the winter months, December, January and February, and the other (No. 2), the same for the year. The number of inches along the course of each line is marked on the line.*

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* Vol. ix, p. 312. Further, vol. x, p. 385, and vol. xiii, p. 80. The connection between the distribution of the ice and the amount of precipitation is appealed to also in ibid., v, 206, 1873, and illustrated from Mr. Schott's chart.

It will be observed on the chart of the *winter* precipitation (No. 1) that the area of *two* inches or less for the precipitation of the winter months spreads into Western Iowa and part of Minnesota. On Mr. Schott's complete chart, it is shown to cover the whole area (some mountain ridges not being considered) from Central Kansas nearly to the Sierra Nevada; from Iowa and Minnesota to Salt Lake City; and from Minnesota northward and westward. The line of *four* inches, as the copied portion of the chart shows, passes along the eastern border of Kansas, and through Central Iowa and Wisconsin, and thence northward. It runs north of Salt Lake City near the line of *two* inches for 150 miles, and thence—while the latter diverges eastward—continues northward along the summit of the mountains.

In contrast with this, the winter precipitation over New England is 8 to 12 inches; over New York, 6 to 10 inches; over Ohio and Indiana, 8 to 10 inches; over the Southern States, from Virginia to Georgia and Louisiana, 10 to 20 inches.

On the chart No. 2 is given the amount of *annual precipitation* for this same region. While this amount is 40 to 45 inches in New England; 40 to 50 inches from Pennsylvania southwestward; and 40 to 45 over Ohio and Indiana, the line of 20 inches (half the average for New England, Pennsylvania, Ohio, and less than half for the more Southern States) crosses Western Minnesota and passes just west of Iowa; and the line of 16 inches enters Minnesota. Mr. Schott's chart shows that the area over which the precipitation is only 12 inches or less up to 16 inches embraces (exclusive of parts of the Rocky Mountain summit not considered) the wide area from the meridian of 96° in Minnesota and from that of 98° in Southern Dakota, to the meridian of 121° in Oregon, or 23 to 25 degrees in latitude; and from the meridian of 102° in Nebraska and Kansas to Virginia City in Nevada, near the meridian of 117°. North of the United States the lines are not given; but it is well known that the breadth of the area does not diminish in that direction.

At the same time it is to be considered that the *isothermal* lines, and especially those of the summer, bend far northward over the dry region. The climate consequently would have necessarily occasioned over this central and western area of the continent, only a small amount of precipitation in the Glacial era; and all the observed glacial facts prove positively that it

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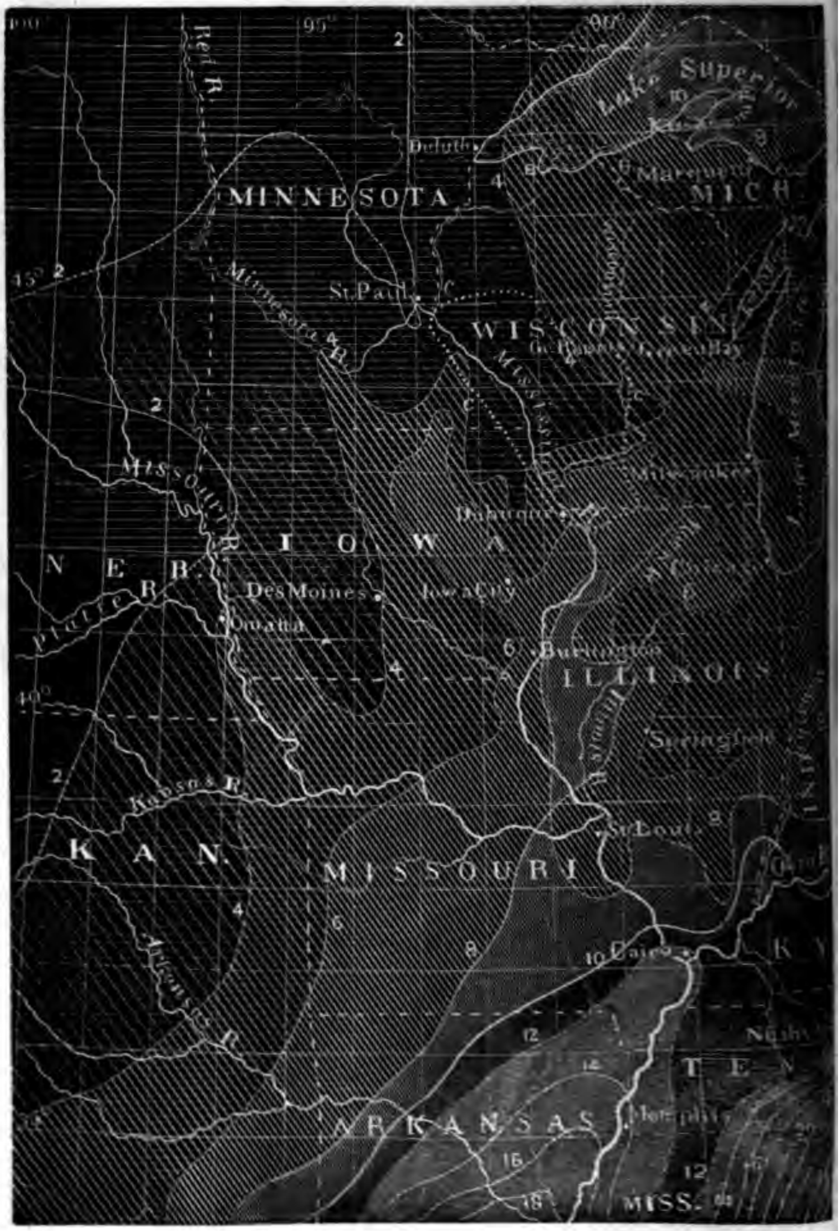
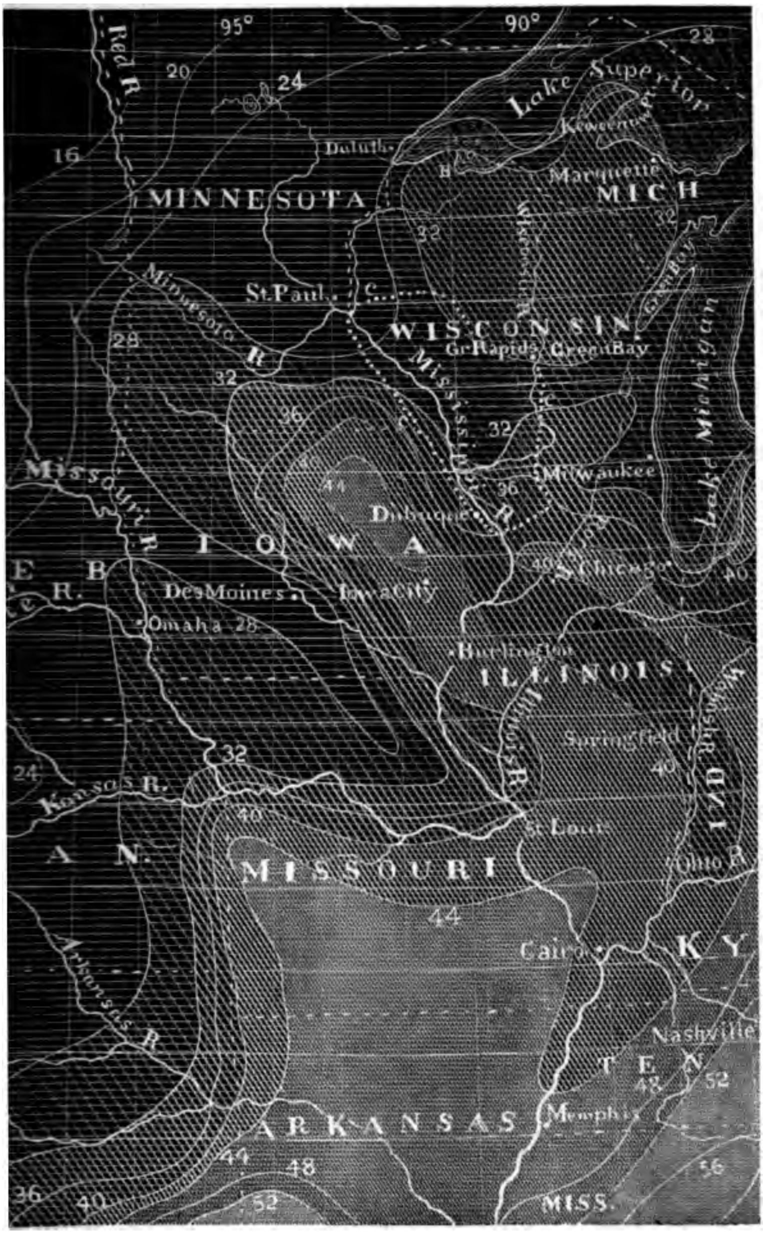
* In the western portions, the line 2 of No. 1, and 16, 20 and 24 of No. 2, are, according to Mr. Schott, only approximations.



1. LINES OF EQUAL WINTER PRECIPITATION.



2. Lines of Equal Annual Precipitation.

was too small for the production of a southward moving glacier. The southwestward direction of the scratches and of the bowlder-movement over the area from Wisconsin to Lake Winnipeg not only sustains this, but shows also that the ice had its greatest height over the region of greatest precipitation somewhere between the line from Wisconsin to Lake Winnipeg and beyond, and that of the Atlantic coast. More facts are needed before the northern limit of the glacierless and driftless area can be laid down.

2. *Driftless area in Wisconsin.*

The charts accompanying this paper have been introduced here partly to exhibit the bearing of the climatal facts on the question as to the origin of the "driftless area" in Wisconsin, a description of which is given, from Professor Irving's Report, on page 61 and beyond of this volume. The outline of this driftless area is indicated on the two maps (Nos. 1 and 2) by a dotted line, lettered *c*, *c*, *c*, *c*. It is a striking fact that on the *winter* chart (No. 1) the driftless area is almost wholly included within the area which has only 2 to 4 inches for the amount of winter precipitation; that this area of 2-to-4 inches fall extends in that direction like a deep bay between areas of greater precipitation. To the northeastward of it the amount of precipitation increases from 4 to 10 inches, the last being that on Keweenaw Point, and southward and southwestward over Illinois and part of Iowa to 6 and 8 inches. All of the Wisconsin driftless area, excepting its southern portion, is within the 2-to-4 area. Again, on the chart of *annual* precipitation (No. 2), this driftless area (excepting its south end) is the driest part of Wisconsin, the amount of precipitation over it being below 32 inches, while it is 32 inches or over to the northeast, as far as Keweenaw Point, and to the south and southwest.

It seems therefore probable that the driftless area was located to a large degree by the climate. At the same time Professor Irving's suggestion is needed to make the explanation complete. For this area was not a continuation southeastward of the great central driftless area of the continent, as it is hygrometrically, but, on the contrary it was wholly surrounded by moving ice; for the ice is proved by bowlders to have extended on the *west side* of the area from the southwest end of Lake Superior over Minnesota and Iowa, and even beyond into Missouri where large bowlders occur. The locality of the mass of native copper found in Lucas County, Southern Iowa, is distant four hundred and sixty miles from Keweenaw Point, its probable source. From Iowa the ice stretched eastward across Illinois to the Lake Michigan region.

This southwestward prolongation of the glacier from the western half of Lake Superior over a region as dry as that of the Wisconsin driftless area appears to be a consequence, as Professor Irving urges, of the great depth of the Lake Superior trough—over a thousand feet below the present surface of the water—and its lying in a southwest-by-west (or about S. 55° W.) direction, which was nearly that of the glacier motion in that part of North America. For this would have determined the movement of a great ice-mass southwestward, over the northern half of Wisconsin, and over Minnesota, and thence across Iowa, where there was again abundant precipitation. The annual precipitation is 32 to 40 inches over Eastern Iowa, Illinois, and the region about the southern part of Lake Michigan.

With such a driftless area within the glacier limits—an area without depth of ice enough for movement—the pitch of the upper surface of the ice around it would have been quite various. The height must have been great enough in the eastern part of the Lake Superior region and north of Keweenaw Point to have determined movement through Minnesota and Iowa to Missouri, a distance of five hundred miles, this being shown by the bowlders of copper. Whatever the pitch along that course, it was twice as great toward the Wisconsin driftless area, since the northern border of the area is hardly half as far. Down Lake Michigan the pitch continued into Illinois and Indiana; but the Kettle Range west of Lake Michigan, running along the east front of the driftless area, marks out, as Professors Chamberlain and Irving show, its moraine termination in that direction.

The eastern parallel branch of the Kettle Range lying between the Green Bay Valley and Lake Michigan, which, according to these geologists, is also a moraine ridge, is evidence, as they observe, that at the time when it was formed, the glacier of Green Bay Valley was distinct from that of Lake Michigan. It seems probable that when the Glacial era was at its height, the two were merged in one glacier; but that later, as the ice diminished, the former became independent, and that then the eastern Kettle Range was made.

3. *The earth's axis had the same position in the Glacial era as now*, if the driftless character of the Wisconsin area depended on the climatal conditions explained. The concordance between the limits of the drier areas of the Glacial era and those of the present time, and especially the fact in this respect with regard to the isolated area in Wisconsin sustains this proposition. The probability that such was the truth was long since made apparent by the observation that the southern termination of the glacier in North America and Europe was very nearly along what is now the course of the same identical isothermal. The position and extent of the Wisconsin driftless area afford more precise and positive evidence.

ART. XXXVI.—*The Ancient Outlet of Great Salt Lake;* a letter to the Editors, by G. K. GILBERT.

GREAT Salt Lake has no outlet, and its fluctuating level is determined by the balance between inflowing streams and solar evaporation. On the surrounding mountains there are water-lines rising in steps to a thousand feet above its surface, and showing that in ancient times a great body of water occupied its basin. This ancient body, known as Lake Bonneville, was 345 miles long from north to south and 135 miles broad, and its vestiges are on so grand a scale that they have attracted the attention not only of geologists but of every observant traveler. It naturally occurred to many persons to enquire whether the lake waters did not in their flood stage find an outlet, and several theories have been advanced in regard to it; but previous to 1876 the outlet was not discovered, or if discovered its position was not announced. In the summer of that year I left Ogden for the purpose of seeking the outlet at the north, and in a few days had the great pleasure of finding it in Idaho, at the north end of Cache Valley, the locality being known as Red Rock Pass. The circumstances were such as to leave no doubt in my mind that I had determined the actual point of outflow, and on my return to the East I made the announcement without reservation in a communication to the Philosophical Society of Washington. The announcement was also made for me in the same unequivocal manner by Professor Joseph Henry, in the Smithsonian Report for 1876 (p. 61), and by Professor J. W. Powell, in Baird's Annual of Scientific Discovery for 1876 (p. 260), and there seemed no occasion for further publication until the matter should receive its full discussion in the Reports of the Survey of which Professor Powell has charge. But, in the American Journal of Science for January, 1878 (p. 65), there appears a statement (apparently on the authority of Dr. F. V. Hayden, but without signature) that "it is believed that the explorations of the Survey, under the direction of Dr. Hayden, the past season, have determined the probable ancient outlet of the great lake that once filled Salt Lake Basin;" and there is so much doubt implied by the use of the phrases "it is believed" and "probable outlet" that it seems proper for me to defend my positive assertions by setting forth the facts which appear to me to place the existence and position of the ancient outlet beyond question.

If Lake Erie were to dry away, and a geologist of the future should examine its basin, he would easily trace the former shoreline around it. At two points he would find this line interrupted. At Detroit and at Buffalo he would meet with narrow, trough-like passes, depressed somewhat below the level of the shore line, and leading to other basins. Following the Detroit Pass he would be led to the Huron basin and would find there a shore line so nearly on a level with the Erie that he could not readily determine which was the higher. Following the Buffalo Pass he would find a continuous descent for many miles to the Ontario basin, and in that basin he would find no water-line at the level of the Erie shore. In each case he would learn from the form of the passage that it had been the channel of a river, and in the latter case he would learn from the direction and continuity of descent, and from the absence of corresponding shore lines, that it had been the channel of an *outflowing* river.

So in regard to Lake Bonneville. To discover its outlet it was necessary to find a point where the Bonneville shore line was interrupted by a pass of which the floor was lower than the shore line, and which led to a valley not marked by a continuation of the shore line. These conditions are satisfied at Red Rock Pass, and, in addition, there is a continuous descent from the pass to the Pacific ocean. All about Cache Valley the Bonneville shore line has been traced, and it is well marked within a half mile of the pass. The floor of the Pass at the divide is 340 feet below the level of the shore line, and its form is that of a river channel. The gentle alluvial slopes from the mountains at the east and west, which appear once to have united at the pass, are divided for several miles by a steep-sided, flat-bottomed, trench-like passage, a thousand feet broad, and descending northward from the divide. At the divide Marsh Creek enters the old channel from the east, and turning northward runs through Marsh Valley to the Portneuf River, a tributary of the Columbia. In Marsh Valley the eye seeks in vain for the familiar shore lines of the Salt Lake Basin, and the conclusion is irresistible that here the ancient lake outflowed.

At the divide a portion of each wall of the ancient channel is composed of solid limestone, and its floor is interrupted by knolls of the same material. It is evident, too, that the channel has lost something in depth, for Marsh Creek and some smaller streams at the south have thrown so much debris into it as to divide it into several little basins occupied by ponds and marshes. It is not improbable that twenty or thirty feet have thus been built upon the floor and that the original bed of the channel where it crosses the limestone is 360 or 370 feet lower than the highest Bonneville beach. Still we must not suppose that the floor of the outlet was ever 370 feet below a coexistent level of the lake, but rather that during the existence of the outlet its channel was slowly excavated to that extent, while the lake was to the same extent drained. This view is sustained in a very striking manner by the phenomena of the shore lines.

From the highest shore line, known as the "Bonneville Beach," down to the level of the modern lake, there is a continuous series of wave-wrought terraces recording the slow recession of the water. As many as twenty-five have been counted on a single slope. Some are strongly marked and others faintly, and some that are conspicuous at one point fail to appear at other points; but there is one that under all circumstances asserts its supremacy and clearly marks the longest lingering of the water. It has been called the "Provo Beach," and it runs about 365 feet below the Bonneville Beach. When the discharge of the lake began, its level was that recorded by the Bonneville Beach. The outflowing stream crossed the unconsolidated gravels that overlay the limestone at Red Rock Pass, and cut them away rapidly. The lake surface was lowered with comparative rapidity until the limestone was exposed, but from that time the progress was exceedingly slow. For a long period the water was held at nearly the same level, and the Provo Beach was produced. Then came the drying of the climate, and the outflow ceased; and slowly, with many lingerings, the lake has shrunk to its present size.

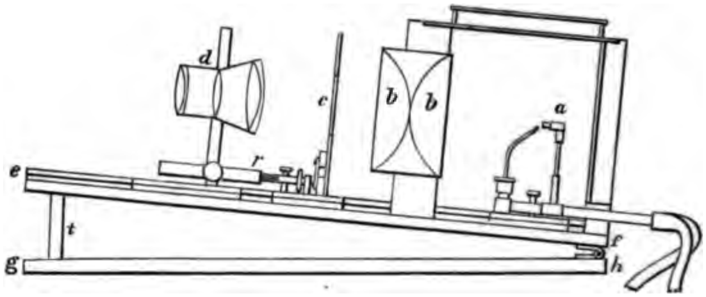
In Dr. Hayden's Preliminary Report of the field work of his Survey for the season of 1877, noticed on page 56 of the current volume of this Journal, there is no mention of the observations at Red Rock Pass, but the omission appears to have been accidental, for on page 7 he says: "At the divide between the Malade and Marsh Creek is another of the old outlets of the ancient Salt Lake when its waters were at the highest level." This passage occurs in a summary of Dr. A. C. Peale's geological observations, but it is to be hoped that the idea will not be advocated in that gentleman's report. The divide referred to is near Malade City, and separates Malade Valley from Marsh Valley. The Bonneville Beach is well marked all about Malade Valley, and nowhere more strongly than in the vicinity of Malade City. It runs between that place and the divide at an altitude of about 400 feet (by barometer) above the city, while the divide, as determined by Dr. Hayden's assistants, has an altitude above the city of 950 feet. After making every allowance for the errors incident to barometric determinations of altitude, it must be conceded that the divide is several hundred feet higher than the water line. It appeared so evident from a distant view that the lake did not overflow this ridge, that I did not ascend to the summit, although I had undertaken last summer to examine every divide between the Columbia and Salt Lake Basins that might possibly have afforded passage to the water. I am aware that Prof. F. H. Bradley, who visited the locality in 1872, expressed the half formed opinion that it had been a point of outflow, but he described no channel of outflow; and it is evident, moreover, that he gave little thought to the subject, for he made the somewhat astonishing suggestion that four outflowing streams might have coëxisted—one at the Soda Spring Pass, one at Red Rock, one near Malade City, and one at the head of the Malade River. If he had seen the channel at Red Rock, I do not doubt that he would have recognized it as the real avenue of discharge.

It is proper to add in this connection, that I have been able to demonstrate that certain small orographic movements have transpired in the Bonneville Basin, not only since its desiccation but during its flooding, and that it is perfectly conceivable that such movements shifted the outflow from point to point. To ascertain whether they actually did so, I have traced out during the past summer all of the shore line that had not previously been explored, and in so doing I have satisfied myself that the only outlet of Lake Bonneville was at Red Rock Pass.

ART. XXXVII.—*On the Projection of Microscope Photographs*; by JOHN CHRISTOPHER DRAPER, M.D., LL.D., Professor of Natural History in the College of the City of New York.

IN the lanterns that are constructed for the projection of photographic or other images on a screen, the support or stage on which the photographic slide is placed is close to, and at an invariable distance from, the condensing lens. So long as the objects to be projected are nearly equal in size to the diameter of the condenser, this is the only adjustment that can be made to illuminate the whole surface of the object; but, when the diameter of the field occupied by the object is only one-half, or one-quarter of the diameter of the condensing lens, the brilliancy of the result obtained upon the screen may be greatly increased by removing the supporting stage or object carrier to a greater distance from the condenser, so that a convergent beam of light may fall on the object to be projected. To accomplish this I have constructed the following form of lantern:

In the figure, *a* is a zirconia light, mounted on an adjustable base (see American Journal of Science and Arts, Sept., 1877, page 208), which may be used with a condensing lens of very short focus, since the zirconia is not burrowed into cavities where the oxyhydrogen flame impinges, as happens with lime cylinders, and causes the flame to be reflected upon the condensing lens and thereby destroys it. In the jet employed, the gases are mixed just before they are ignited. *b*, *b*, is a short focus condensing lens. *c*, the stage or support carrying the photographic or other design to be projected. *d*, the projection lens formed of three sets of lenses and giving a perfectly flat rectilinear field. *a*, *c*, *d*, are mounted on a base board *e*, *f*, to the end of which the lantern box *a*, *b*, is attached, and which is freely open above and below to permit perfect ventilation. The base carries lateral grooves in which *a*, *c*, *d* slide, allowing them to be placed at varying distances from *b*, and fixed by suitable binding screws. *c* and *d* are also connected together by a rod *r*, carrying an adjustment screw at *r*, by which the change of distance between *d* and *c*, required in giving the correct focus, may be obtained. The base *e*, *f*, is attached to a second or



under base *g*, *h*, by a hinge at *h*, which allows the end *e*, of the movable base *e*, *f*, to be raised to any required angle; at which it may be maintained by the block at *i*. So convenient and compact is this lantern that it may easily be stowed away in a small trunk.

When a series of objects of very different sizes is to be projected, as is the case with microscopic photographs taken under the same adjustments, it is, as we have said, a great gain in the projection of small objects if the circle of light used for illumination is reduced; and, at the same time, increased in brilliancy. This is accomplished in the above arrangement as rapidly as can be desired by removing *d*, *c*, together, along the slide of the base *e*, *f*, to a sufficient distance from the face of the condenser *b* to allow the convergent rays from the latter just to cover a circle which will include the object to be projected. The greater intensity of the illumination thus obtained renders the definition of fine markings or other peculiarities on small objects as clearly visible at considerable distances as are the coarser markings on large objects under a weaker light.

In closing this brief communication I desire to add that I have made photographs of *Frustulia saxonica* under a power of 7,000 diameters. The photographs in question were made in the City College building by a one-twentieth inch immersion Beck lens. The light was from the sun, reflected by a heliostat, through ammonio-sulphate of copper solution, and condensed on the object at an angle of 30° to 40°. The photograph in question was direct, by which I mean that there was no intermediate or secondary enlargement of a first photograph. With this photograph and the lantern described, I have shown *Frustulia saxonica* magnified more than half a million diameters; a result which must be seen to be appreciated.

ART. XXXVIII.—*On the Discovery of Lower Silurian Fossils in Limestone associated with Hydromica slates, and on other points in the Geology of Lehigh and Northampton Counties, Eastern Pennsylvania;* by FREDERICK PRIME, Jr., Professor of Metallurgy at Lafayette College, Easton, Pennsylvania.

[Professor Prime's discoveries were made in connection with his work, during the past season, as Assistant Geologist in the Geological Survey of Pennsylvania. The region in which the discoveries were made lies near the eastern boundary of the State, southeast of the Blue Ridge. The rocks are related in position, in lithological character, and also in their direct association with large beds of limonite, to those of Vermont in which the discoveries of fossils by Mr. Wing were made, and to the continuation of these Vermont rocks southward through Berkshire, Massachusetts, near and along the Taconic range. The discoveries were first made known to the writer by Professor Lesley, the able director of the Pennsylvania Survey, and by Professor Prime, in November last; and both recognized the intimate geological as well as geographical relation between these East-Pennsylvania and West-New England rocks. Professor Lesley stated in his letter that the opinions which he had derived, from the observations of others, more than thirty years since with regard to New England geology, he now (since the discovery of fossils in limestones among the metamorphic rocks of Vermont, of Bernardston and Littleton in the Connecticut Valley, and of Eastern Pennsylvania) regards as greatly strengthened in probability—namely: That Paleozoic rocks make up the Green Mountains, and also the White Mountains, and that the latter include beds of Devonian age. Professor Prime's article, which is here published, is cited from the Proceedings of the Philosophical Society for December 21, where it appears under the title "*On the Paleozoic Rocks of Lehigh and Northampton Counties, Pennsylvania.*—J. D. D.]

THE Paleozoic rocks of Lehigh and Northampton counties are: The Potsdam Sandstone (No. I); Magnesian or Auroral Limestone (No. II); Trenton Limestone (No. II); Utica Shale (No. III); Hudson River or Matinal Slate (No. III).

The Potsdam sandstone is first found in the outlying peninsula of the South Mountains, known as Lock Ridge, where it occurs on the northwest flank of the hill and undoubtedly has a northwest dip. It next occurs in two small patches on the northern flank of the main range of the South Mountain near Macungie (formerly Millerstown). A small patch of it is also found associated with the gneiss, where the latter crops out through the limestone in the gorge of the Little Lehigh Creek at Jerusalem Church, two miles northwest of Emaus. But it is first seen to any great extent along the north flank of the main range just south of Emaus, where its occurrence is constant, but of varying thickness, and continues for a distance of four and a half miles, after which it can no longer be traced.

It occurs again at the ridge of the South Mountain, close to Allentown, which forms the southern barrier of the Lehigh River, between Allentown and Bethlehem, where the sandstone is about twenty-five feet thick and extends with a few intervals (where it has been cut out by the river) the entire distance between these two places. It also extends across the Lehigh and forms the capping rock of a portion of the gneiss just east of Allentown and north of the Lehigh. The contact between the gneiss and sandstone is distinctly seen about two miles east of Allentown on the Lehigh Valley Railroad track.

The very lowest beds of the Potsdam sandstone are actual pudding-stones, containing quartz pebbles the size of a man's fist and larger, and fragments of red, unaltered orthoclase. The upper beds are composed of a hard, compact quartzite containing greater or less quantities of feldspar nodules, which weather out and impart to the rock a pock-marked appearance. When first quarried the color of this quartzite is blue to bluish-gray, which on exposure soon changes to a dark reddish-brown, due to the oxidation of the ferrous oxide it contains. The change from a pudding-stone to a compact quartzite in the sandstone shows that there has been a gradual sinking of the earth's crust and an increase in the depth of the sea, thus preparing the way for the subsequent deposition of the limestone.

The Potsdam sandstone often, as elsewhere, contains *Scolithus.*

Next above the Potsdam sandstone occur *hydromica slates*, which Rogers has called the Upper Primal Slates, but *which really form a portion of the No. II limestone*, and gradually pass into this. They lie along the north flank of the South Mountain and overlie the Potsdam conformably wherever this is visible, being far more persistent in their occurrence, continuing with few intervals the entire distance from the western boundary of Lehigh county to the Delaware River. They are of great economic importance as carrying the lowest range of brown hematite iron ores, to be mentioned later.

These slates are composed in great part of the mineral damourite and occur of a pink, gray, white and yellow color.

When exposed to the weather they very rapidly decompose to soft unctuous plastic clays in a few days, and some of these will in time probably become valuable in the manufacture of coarse kinds of pottery. Generally they contain more or less of the carbonates of lime and magnesia, and silica, mixed with the damourite.* Hydromica slate also occurs the greater portion of the distance from the western boundary of Lehigh county to the Delaware River, at the junction of the No. II limestones with the No. III slates, here also carrying brown hematite ores in extensive deposits.

It also occurs intercalated in the limestone, forming layers from the thickness of a sheet of paper to several feet, and these layers are innumerable. Their existence has been seen both in rock outcrops as well as in wells which have been sunk.

The clay to which the hydromica slate decomposes is generally of a white color, although sometimes brown from the presence of hydrated ferric oxide. Analyses would seem to show that the clay contains rather less potash than the undecomposed rock.

Overlying the hydromica slates, and conformable with these and the Potsdam sandstone, is the No. II or Magnesian limestone (Auroral of Rogers), which extends as a great mass varying from six to seven and a quarter miles in width. At four points gneiss crops out through the limestone. These are at Chestnut Hill north of Easton, at a hill two miles north of Bethlehem, the gneiss ridge north of the Lehigh, between Allentown and Bethlehem, and at Jerusalem Church, two miles north of Emaus. Otherwise its continuity is unbroken.

In its lower beds the limestone contains large quantities of chert, forming nodular masses of very various sizes and usually having their longest axes conformable to the bedding of the enclosing rock. This chert occurs in the manner described by Safford† as characteristic of the Knox dolomite of Tennessee. It disappears, however, in the upper strata.

The limestone varies from a blackish-blue to dove color, being for the most part compact to semi-crystalline, while there are occasionally shaly beds. In composition it varies much, often approaching a true dolomite, again a pure limestone. But from the isolated analyses made it would seem as if the percentage of magnesia was less in the upper beds than the lower ones. The limestone is always siliceous, often very much so, and hence much care is now being taken by many of the ironmasters in selecting beds of it, which are low in silica, so as to be suitable for smelting the iron ores of the Great Valley and New Jersey, which are high in silica. It often contains minute grains of pyrite disseminated through it, which weather out on exposure, leaving minute cavities behind. Numerous analyses have shown the presence of ferrous carbonate varying in amount from 0·538 to 1·305 per cent.

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* Report of Progress for 1874 of Lehigh Dist. Geol. Survey of Penn., p. 12.

† See Geology of Tennessee, by Safford, pp. 215, 218.

A peculiarity of the limestone is that it is often brecciated, the fragments being composed *exclusively of limestone*, cemented together by calcite or dolomite. The brecciated appearance is rarely visible on fresh fracture, being usually brought to view by weathering. When seen in place it will usually be found that one or more brecciated beds occur between two others which do not exhibit this peculiarity. As the beds of the No. II limestone have been much disturbed by the force which elevated the South Mountain range, the probable explanation of this brecciation is that a very hard, unyielding bed occurs between two more pliable ones; that these, when subjected to the lateral thrust of the uprising mass of the South Mountains, have conformed themselves to the folds of the strata, while the harder one, being unable to do this, has been fractured and *re-cemented in sitû* by the percolation of calcareous waters.

Some observers have supposed that the No. II formation is actually composed of two limestones, the lower one belonging to the Huronian, the upper to the Calciferous; and patches of the latter are supposed to overlie the former. The upper limestone (according to these observers) having been formed from the lower, the brecciated limestones are adduced as evidences of upheaval and shore action.

The explanation I have offered of the formation of the brecciated limestone is both more in accordance with the facts observed and with the generally accepted view of the deep-sea formation of limestone than the hypothesis above stated; for the brecciated limestones are as common near the base of the series as at the top.

Besides, the genus *Monocraterion* found in the Lehigh county limestone belongs to the same family as *Scolithus*, and is therefore no greater proof of age than the latter; and it occurs in but one locality close to the top of No. II, being not more than fifty to one hundred feet from the overlying Calciferous and Trenton.

The fossils thus far found in the No. II limestone do not number a dozen specimens, and have been found in but four localities. At Helfrich's Spring, about two and a half miles north of Allentown, the Jordan makes a great bend around a limestone hill, and, by an underground passage of a portion of its waters, has excavated a cave a short distance into it. At the west end of the hill, near the small opening where that portion of the creek forming the spring disappears, there occurs a new species of *Monocraterion*, as yet undescribed. Of this half a dozen casts have been found; but all efforts to discover the fossil itself have been hitherto unsuccessful. This discovery is the more interesting as the genus *Monocraterion* has hitherto only been known to occur in Sweden.

About half a mile northeast of this five or six specimens of a *Lingula* were found in John Schadt's quarry, but it is impossible to determine its species. About half a mile west of Helfrich's Spring a single specimen of an *Orthoceratite* was found close to the Jordan, just north of Scherer's Tavern, but so imperfect that its species is undeterminable. Finally a specimen of *Euomphalus* was found on Nero Peters' farm, two miles east of Ballietsville.

Not a single fossil has thus far been found in the No. II limestone of Northampton county.

The No. II limestone, like the Magnesian limestone of the Mississippi Valley, is exceedingly soluble. Streams constantly disappear in the ground, forsaking their original beds except when the volume of water is too great to be carried off by the subterranean channels, only to reappear again as springs at greater or less distances. The effects due to this solution of the limestone are very great. Not only are small sink-holes very common, but beds are found often much contorted locally in a manner which can only be explained by supposing them to have dropped down by their own weight into caverns excavated by the water beneath them. Possibly also the contortion of the hydromica beds as developed in the brown hematite mines at the junction of the limestone with the No. III slates is due to the same action, rendered more prominent by the passage of streams from the slate to the limestone, where the solving action could begin. The different beds too are soluble in very different degrees; some apparently yield at once to the eroding action of water, while others afford a resistance to this operation for reasons as yet unknown, but which are probably rather mechanical or physical than chemical. Knowing as we do so little of the conditions under which the different layers of limestone, almost or quite identical in composition, were formed, we can only speculate that those layers which resisted erosion were more compact, hard, and dense, perhaps more metamorphosed by a subsequent crystallization than the others, while we actually have no facts on which to base such theories. No better illustration of the darkness amidst which geologists are seeking light can be given than by stating that we are in complete ignorance of the causes which produce different layers of limestone, almost identical in composition, the one above the other. We can explain alternations of shale, sandstone and limestone by changes in depth of the sea in which they were formed; but such an explanation does not hold good where the same rock continued to be formed. Why should the sediment, whether chemical or mechanical, have formed a continuous layer an inch to several feet in thickness, and then a break in continuity have occurred, to be succeeded by another layer of the same material?

While the greater portion of the limestone has in all probability been formed in deep water, we have one instance in a quarry at Uhlersville on the Delaware where it must have been formed as a beach, since we find here distinct traces of ripple marks along the entire face of the quarry, some sixty feet high and fifty feet deep, the strata being tilted nearly vertically.

It has been generally supposed that the limestone dips almost universally southward; and while this view holds good for Northampton county, except at the junction of No. II with the No. III slates and along the north flank of the South Mountains, it is not the case in Lehigh county; for here we find northwest dips, more especially along an axis which is prolonged some distance into Northampton county, a short distance above Catasauqua.

As a general thing the limestones pass conformably under the No. III slates, and the few exceptions where the slates dip toward the limestones, and the latter away from the slates can readily be explained by an overturning of the beds toward the south, by which means as in the slate quarry close to and south of Ironton the slate apparently passes conformably below the limestone.

Overlying the No. II limestone occurs the Trenton limestone which is more fossiliferous and contains such characteristic fossils as *Chætetes lycoperdon* and *Orthis pectinella* as well as the stems of an encrinite. It was first found about a mile south of Ironton in Lehigh county, then at intervals between Bath and Martins Creek in Northampton county; but all attempts to trace it as a continuous formation have thus far been unsuccessful owing to the lack of outcrops. It occurs most extensively at Martins Creek on the Delaware, at a point a little south of the cotton mill, and is there as elsewhere apparently conformable with the underlying Magnesian limestone.

This limestone resembles in appearance the No. II, being however more compact and not at all crystalline, and of a gray black color.

There has been no apparent sudden break between the two, but the transition has been a gradual one. This was to be expected if the subsidence of the sea-bottom was steady and slow. An examination of the beds between Ironton in Lehigh county and the Delaware River, as close to the junction of the limestone and slate as possible, has shown that the limestone for the entire distance is more or less a hydraulic one, due to the greater proportion of alumina which it contains. This also was to be expected if the subsidence continued, as signaling an approach to the era of slate-formation and open-sea deposition. These limestones are utilized on the Lehigh river in the manufacture of hydraulic cements and lately Portland cement has been made at the Copley Cement Works, which is said to be nearly or quite equal to the imported. Careful search and the demand for it will no doubt cause this variety of the limestone to be explored at various other points in the two counties, and will in time render us independent of the cement now sent from the Hudson River. The limestone is of a dull, earthy appearance, entirely free from any crystalline texture and of a dark gray color.

Before closing our discussion of the limestone it is necessary to speak of the large and numerous deposits of brown hematite iron-ore which occur in it, and which form the main support of the extensive iron furnaces of the Lehigh and Schuylkill Valleys.

The brown hematite iron-ore occurs almost exclusively in two irregular lines of deposition; the one along the northern flank of the South Mountain Range, the other at or near the junction of the No. II limestone with the No. III slates. A few other localities, at which the ore is found, but these are insignificant in number compared to the two lines mentioned. Along both these lines the ore is always found either in hydro-mica slate, or resting on limestone very greatly impregnated with damourite; the same is true elsewhere whenever the brown hematite is found *in loco originali*. Some deposits are however found which have evidently been pockets or cavities in the limestone into which the masses of limonite have been forced together with gravel and clay during the Drift Period. Leaving these out of consideration as of minor importance, let us consider briefly those iron-ore deposits which occur in place. It is at once evident that like the rocks with which they are associated they are of secondary origin, and have been derived from still older formations. The occurrence of the brown hematites with silica, alumina, lime, magnesia and the alkalies, more especially potash, points to their having been derived from Archæan rocks containing orthoclase and either hornblende or pyroxene. From the decomposition of these three minerals we are able to derive all the oxides above mentioned including the iron which was without any doubt derived in great part from the decomposition of ferrous silicate present in the hornblende and pyroxene, while a portion of the iron may have been derived from iron pyrites, although this supposition is entirely unnecessary. It is extremely improbable that the brown hematite was derived from the peroxidation and hydration of magnetic iron ore, when we recall the great resistance which the latter offers to chemical change of any kind when exposed to the action of air and water, and its unaltered condition and fresh, bright appearance in rivers and on the seashore. But the question as to how the brown hematite got into its present condition and whether it was deposited cotemporaneously with the rocks containing it, or subsequently to these, is still an enigma and various theories have been offered in explanation. For a resumé of some of these hypotheses reference may be made to a recent article by Professor J. D. Dana, in this Journal, III, vol. xiv, page 136. The almost entire freedom of the hydromica slate, when fresh, in Lehigh and Northampton counties from ferruginous minerals will prevent our having recourse to pyrite, pyrrhotite, chlorite, garnet, mica and staurolite, which Professor Dana says occur in the hydromica region of Connecticut. Hence we must have recourse to other sources. It seems most doubtful that the mineral, from which the brown hematites were derived, was deposited cotemporaneously with the hydromica slates in the district under discussion, since we find the ore often passing through the slate or clay obliquely and intersecting the bedding. It is more probable that the ore was conveyed to its present position by infiltration subsequent to the formation of the hydromica slates. Whence was it derived? I have already stated that the limestone contains varying proportion of ferrous carbonate and of pyrite, and when we consider the enormous erosion which the limestone has undergone, the wonder is not that the deposits of iron ore should be so great, but rather that they should be so small. The ferrous carbonate and the pyrite oxidised to ferrous sulphate being both soluble in water, the former when the water contains carbon dioxide, the waters would naturally carry these salts in solution until they came in contact with precipitating agents such as the alkaline silicates which the hydromica slates carry. These last became converted to carbonates and sulphates, leaving the iron behind, either directly as hydrated ferric oxide, or possibly as ferrous silicates which became later decomposed by the action of aerated water to hydrated ferric oxide and free silica, which latter we now find so universally associated with the brown hematites as quartz. Whatever the origin of these ores may have been, one thing is evident, viz., that there is some genetic relation between the brown hematites and the hydromica slates, as evidenced by the almost universal occurrence of the ore in the slate, extending all the way from Vermont to East Tennessee through the Great Valley as well as in the interior valleys of Pennsylvania where the No. II limestones occur.

It is well here to emphasize the fact that these brown hematite ores all belong to the Lower Silurian limestone formation, since, in 1874, Dr. Sterry Hunt, after a cursory examination of Ziegler's Mine in Berks County, situated at the junction of the No. II limestone and the No. III slates, made the mistake, in a paper on "The Decay of Crystalline Rocks" before the National Academy of Science, of supposing that the hydromica slates belonged to the Huronian Period:—a mistake into which so eminent an observer as himself would never have fallen had he been better acquainted with the region.

At intervals along the junction of the limestones and slates there occurs a black carbonaceous shale, often decomposed to black or dark blue clay, which I have supposed to be the representative of the Utica shales. It consists of a very carbonaceous hydromica slate (containing damourite), without any fossils and may not belong to the Utica Period at all. In no instance has it been found more than one to twelve feet thick, but it sometimes carries pyrite from which a portion of the iron ores, just mentioned, may have been derived. These shales are of no economic importance.

Overlying these come the No. III, Hudson River or Matinal Slates, which extend into the Kittatinny Mountains. A large portion of these slates are extremely useful for roofing and other household purposes, and extensive quarries have been opened at various points for the purpose of extracting them, as, however, they have been but very slightly examined, during the progress of the present Geological Survey of the State, I shall defer a more detailed description of them to some future time.

ART. XXXIX.—*On the Influence of Temperature on the Optical Constants of Glass*; by CHARLES S. HASTINGS, of the Johns Hopkins University.

A FORMULA connecting the refractive power of a body with its density, established by Newton, is well known. This, though founded upon the assumption of the emission theory of light, is found to hold true for gases and vapors and, qualitatively at least, for most solids and liquids. Still various deviations from the law have long been recognized. Arago showed that water increased in refractive power in changing its temperature from that corresponding to its maximum density to the freezing point. This has since been made the subject of an investigation by Jamin, who gives 0° C. as the temperature of maximum refractive power.

More recently Dale and Gladstone have made an extensive study of the changes produced in the refractive and dispersive powers of various liquids by increase of temperature; a study which has been largely extended by the labors of Schrauff, Landolt and others. In all liquids, with the exception noted above, increase of refractive power accompanies increase of density.

Observations of the same sort upon solid bodies lead, however, to much less accordant results. Rudberg, in investigating the optical properties of several crystals, found that in arragonite and quartz, the variations in density and refractive index are in the same direction, but that with calcite the case is different, the index for the ordinary ray seeming independent of changes in temperature, and for the extraordinary ray the variation is abnormal. This is less striking, however, when we bear in mind the discovery of Mitscherlich that the deportment of Iceland spar in regard to extension by increase of temperature is quite exceptional.

But before Rudberg's investigation, Arago and Neumann had independently pointed out the fact that glass does not follow the ordinary law. Fizeau undertook to determine quantitatively the changes which various glasses undergo, as regards their mean indices of refraction, and devised for that purpose, an elegant and celebrated method. I shall quote later, some of his results, the only quantitative ones which I have been able to find.

The instrument with which the following determinations were made, is the large spectrometer by Meyerstein, belonging to the Physical Laboratory of the Johns Hopkins University. The circle is 12 inches in diameter, divided to 6′, and reads by two microscopes to 2″. The probable error of one division is 1″·48, and its larger periodic errors are expressed in the formula

$$N = 2''{\cdot}386 + 7''{\cdot}82 \sin(z + 62^\circ\ 36') + 2''.1 \sin(2z + 157^\circ) \\ + 4''{\cdot}32 \sin(3z + 328^\circ) + 0''{\cdot}46 \sin(4z + 122^\circ)$$

N being the corrected and z the immediate reading.

Much labor was expended in putting the instrument in so satisfactory a state, for in order to secure uniformity of reading it was found necessary to re-grind the axis. The collimating telescope too, was, by inexcusable carelessness in construction, directed nearly one-fourth of an inch away from the axis, thus vitiating all determinations of double deviations by the introduction of unknown errors of aberration in the object glass.

At the center of the instrument is a small circular platform bearing a graduated silver limb at its edge which is read by two verniers to single minutes of arc. This platform turns independently of the large circle with inconsiderable friction, and upon it is placed the prism to be studied. The methods adopted to adjust the instrument and prism, and to measure the angle of the prism were exceedingly accurate and perhaps find a proper place for description here.

In the focal plane of the positive eye-piece was a reticle consisting of lines on glass ruled as follows:—a system of two pairs of parallel lines crossing at center of field, one pair being horizontal and the other vertical, the angular distance (measured from objective) between the components of each pair being about 1′; and a single line, vertical and about 25′ to the right of the vertical pair. Over this single line was placed a small totally reflecting prism, one of its faces turned toward an opening in the eye-tube. The advantage of this arrangement is at once evident, for it was possible to get a strongly illuminated image of the single line, reflected by a plane beyond the objective, between the two vertical lines, at the same time avoiding the annoyance of waste light reflected from the eye-lenses on the one hand, since the mirror was within the eye-piece, and from the objective on the other, owing to the eccentric position of the mirror.

The instrument was put in adjustment by means of a piece of plane-parallel glass placed vertically upon the platform, i. e., so that its plane was parallel to the axis of the instrument. The platform was then turned with the glass till the reflected image of the reticle was seen in the telescope. The telescope was then sharply focused upon this image and the eye-piece clamped in that position as evidently corresponding to adjustment for parallel rays, at least for rays of medium wave length. Then the glass was turned 180°. If the image of the horizontal lines corresponded with the lines themselves it was good; if not, the correction was divided between the glass and the telescope. Proceeding in this way, it was easy to adjust the telescope so that once sighting the reflected image it would be again visible in the same place by turning the glass 180°, and that independently of the azimuth of the platform as referred to the large circle. When these conditions are met it is clear that the axes of the instrument and platform are parallel, while that of the telescope is perpendicular to both. The proper adjustment of the collimating telescope as regards focus and direction, is so evidently attainable from these that it is not worth while describing it specially.

In order to measure the angle of a prism, it was placed upon the platform in the place of the glass plate and adjusted by the proper screws until both faces were perpendicular to the line of collimation of the telescope; a reading was then made when the image of the single vertical line fell exactly half way between the two wires at the center of the field. The error of setting was found to be generally less than 1″, not a surprising result when one remembers that the three lines were of the same intensity, and that a motion of but 30″ in the prism would carry the reflected line from one of the pair to the other.

The next step was to rotate the great circle, and with it the prism, until the other face took exactly the same position; the reading then would clearly be the supplement of the desired angle; in practice, however, the angle was repeated by turning back the platform independently of the circle, the repetition being carried usually to six times, and then back over the same ground for verification. This method was well adapted to this work as the mass moved independently was small, and turned with little friction. It failed however, as might be expected, in measuring later the angles of deviation, for here the joints to be turned were much larger, and that limit of accuracy which is always reached so easily and quickly by the method of repetition, was not below that of a single reading of the microscopes.

Of the adjustment of the prism for measuring the double angle of deviation for the different Fraunhofer lines, little need be said except that, besides the care taken in placing the surfaces exactly parallel to the axis of the instrument, the prism was shifted on the platform until, seen through the lens of the collimator by looking through the slightly opened slit, exactly the same central portion of the objective was projected upon the faces of the prism in both its right and left positions of minimum deviation. This is a very important precaution in order to eliminate errors due to aberration, which must exist in every lens save for rays of a single wave length, or, in rare cases, of two wave lengths.

The position of the prism for minimum deviation of a single line was carefully determined on the small circle attached to the platform, by experiment, while those of the other lines observed were computed after an approximate measurement of the angle of deviation.

The process followed in making the measurements, was to point the telescope upon the particular Fraunhofer line which was to be determined, with the prism in its first position of minimum deviation and read the circle by both microscopes, then to turn the prism to its second position of minimum deviation and read again, the difference of readings being the double deviation required. With this observation was also put the temperature of the prism at the time. This course was pursued from six to ten times, using in each case a different portion of the circle, and then a new line was undertaken. In order to determine the temperature of the prism, a metal cell was cemented to the prism, the back of which formed one side of the receptacle; in this was placed a small centigrade thermometer reading to quarter degrees, estimated, however, to tenths, and then filled with water. The reading of the barometer was also taken at each series of observations. It is needless to say that each reading of the circle was corrected by an amount determined from the formula given above. The prisms were as follows:

I, Of Feil's flint glass, No. 1237, sp. gr., 3·554.
Angle of prism, 60° 4′ 56″·21±0″·18.
II, Of Feil's flint glass, No. 1241, sp. gr., 3·151.
Angle of prism, 59° 58′ 31″·25±0″·61.
III, Of Feil's crown glass, No. 1219, sp. gr., 2·482.
Angle of prism, 60° 10′ 9″·04±0″·23.
IV, Flint glass, sp. gr., 3·497.
Angle of prism, 50° 7′ 53″·01±0″·20.
V, Crown glass, sp. gr., 2·510.
Angle of prism, 59° 48′ 13″·40±0″·08.

The materials of IV and V have been in my possession a long time, and there is no way to determine what they are except from their optical properties, and the statement of the optician from whom I procured them, that the first is of French and the second of English manufacture. I suppose IV to be a flint glass of Feil's making, while V is doubtless of the so-called "soft crown" of Chance Brothers.

The temperature affecting the observations ranges from 14°·9 C. to 29° C., though most of the lines were measured with a maximum difference of 10° C.

The reduction of the observations on the first three prisms to a temperature of 20° C. and barometric height of thirty inches gave the following indices of refraction:

Temperature, 20° *C. Barometric height*, 30 *inches*.

	I.		II.		III.	
Line.	*n*	*e*	*n*	*e*	*n*	*e*
A	1·615258	±3	1·572464	±6	1·512456	±3
B	1·618706	3	1·575332	7	1·514369	3
C	1·620482	3	1·576815	6	1·515334	3
D_2	1·625421	3	1·580905	6	1·517965	3
5614	1·628001	2	1·583024	6	1·519292	3
E	1·631893	2	1·586214	7	1·521274	3
F	1·637756	2	1·590989	6	1·524182	3
4548	1·643524	3	1·595660	7	1·526981	3
G	1·649086	3	1·600129	7	1·529597	3
h	1·654848	3	1·604749	7	1·532251	3
3951	1·659757	2	1·608658	7	1·534458	3

In this table the first column indicates the Fraunhofer line to which the indices of refraction, in columns headed *n*, refer. The last line is beautifully defined and lies nearly midway between H_1 and H_2. The columns under *e* give the probable errors of each determination in units of the sixth place decimal. It should be remarked here that the larger probable errors in the determinations of prism II arise from the greater probable error in the determination of its refracting angle, hence the values are relatively, though not absolutely, as accurate as the others.

The following table contains, in the columns under k', increase of refractive index also in units of sixth place, corresponding to an increase of 1° C. in temperature.

Line.	I.		II.		III.	
	k'	k	k'	k	k'	k
A	4·12	3·79	1·70	1·74	− ·82	− ·77
B	4·04	4·22	2·35	2·04	− ·30	− ·53
C	3·96	4·45	2·77	2·19	− ·06	− ·41
D_2	5·12	5·07	2·43	2·61	− ·00	− ·08
5614	6·58	5·40	2·56	2·84	+ ·04	+ ·10
E	5·42	5·87	2·68	3·16		+ ·36
F	5·69	6·57	3·52	3·63	− ·09	+ ·74
4548		7·24	4·32	4·09		+1·11
G	8·53	7·85	3·86	4·51	+1·26	+1·44
h	8·03	8·48	4·78	4·93	+1·05	+1·78
3951	9·21	8·99	6·04	5·28	+3·22	+2·06

It is evident at a glance that the values of k' increase in each case with the decrease of wave length; and it was found that the quantities could be embodied, within the limits of error of the observations, by a formula of the form

$$k=\alpha+\beta\left(\frac{1}{\lambda}\right)^2.$$

The significance of the constants α and β is clearly that the first is the change in refractive power for light of indefinitely great wave length, or briefly, the change in refractive power, and the second, the change in dispersive power. The following are the numerical values for the three cases:

$$\text{I}\ldots\ldots\ldots\ldots\ldots k=\ 1{\cdot}875+1{\cdot}110\frac{1}{\lambda^2}.$$

$$\text{II}\ldots\ldots\ldots\ldots\ldots k=\ {\cdot}442+\ {\cdot}755\frac{1}{\lambda^2}.$$

$$\text{III}\ldots\ldots\ldots\ldots\ldots k=-1{\cdot}813+0{\cdot}604\frac{1}{\lambda^2}.$$

From these, the quantities contained in the columns headed k are computed.

The most surprising fact which these results point out is, that the variation in dispersive power attending variation in temperature is relatively enormously greater than that of the refractive power. This has, I believe, escaped attention heretofore. It could hardly have passed unheeded, however, did not a singular relation obtain in the coefficients. The dispersive powers of the three specimens of glass, computed in the ordinary way, are as 9 : 8 : 6 nearly, while the coefficients in uestion are as 9 : 6 : 5 nearly; hence if this relation holds pproximately for all optical glasses, as is probable, an achromatic combination good for one temperature is good for all thers within moderate limits.

Fizeau found, in the researches before alluded to, values for he refractive increment for 1° C., not unlike our own, namely:

Crown glass (zinc), sp. gr., 2·626, $k_D=0·00$.
Common flint glass, sp. gr., 3·584, $k_D=2·6$.
Dense flint glass, sp. gr., 4·14, $k_D=6·87$.

The magnitudes of the quantities k show at once the importance of bserving the temperature of the prism in every accurate determination of refractive indices, neglect in so doing generally viti-ting the fifth decimal place. It may be remarked, too, that rdinary variations in barometric pressure cannot be neglected when it is desired to limit the errors to the sixth decimal place.

The prisms IV and V were not studied to determine the emperature constants as were the others, but were observed t as nearly the temperature of 20° C. as possible, and reduced by means of the constants given above. I add these results here because the indices are given with great accuracy, and he "soft crown," though largely used in photographic instruments, has not been much studied. It should be stated, how-ver, that Van der Willigen has published the constants of a glass which seems nearly identical.

Temperature, 20° *C.* *Barometer*, 30 *inches*.

Line.	IV.	V.
A	1·603945	1·509607
B	1·607306	1·511584
C	1·609041	1·512580
D_2	1·613843	1·515288
5614	1·616333	1·516673
E	1·620103	1·518719
F	1·625751	1·521696
G	1·636702	1·527300
h	1·642284	1·530075
3951	1·647048	1·532381

The probable errors were not computed, for the reason that the emperature corrections were assumed; but as regards their ccuracy, the values under IV are perhaps quite as good as those which go before; those under V, on the other hand, have much larger errors, though probably in every case below wo units in the fifth place. This finds its explanation in the act that the material was not homogeneous, thus giving less perfect definition than the others, though the surfaces of the prism were of the same excellence.

I have made these determinations for the purpose of discussing the theory of the astronomical objective.

Johns Hopkins University, January, 1878.

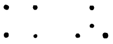
ART. XL.—*A note on Experiments with floating Magnets; showing the motions and arrangements in a plane of freely moving bodies, acted on by forces of attraction and repulsion; and serving in the study of the directions and motions of the lines of magnetic force*; by ALFRED M. MAYER.

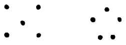
FOR one of my little books of the Experimental Science Series I have devised a system of experiments which illustrate the action of atomic forces, and the atomic arrangement in molecules, in so pleasing a manner, that I think these experiments should be known to those interested in the study and teaching of physics.

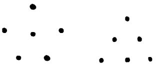
A dozen or more of No. 5 or 6 sewing needles are magnetized with their points of the same polarity, say north. Each needle is run into a small cork, $\frac{1}{4}$ in long and $\frac{3}{16}$ in. in diameter, which is of such size that it just floats the needle in an upright position. The eye end of the needle just comes through the top of the cork.

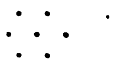
Float three of these vertical magnetic needles in a bowl of water, and then slowly bring down over them the N. pole of a rather large cylindrical magnet. The mutually repellant needles at once approach each other and finally arrange themselves at the vertices of an equilateral triangle, thus .·. . The needles come nearer together or go further away as the magnet, above them, approaches them or is removed from them. Vibrations of the magnet up and down cause the needles to vibrate; the triangle formed by them alternately increasing and diminishing in size.

On lifting the magnet vertically to a distance the needles mutually repel and end by taking up positions at the vertices of a triangle inscribed to the bowl.

Four floating needles take these two forms 

Five " " " " " " 

Six " " " " " " 

Seven " " " " " " 

I have obtained the figures up to the combination of twenty floating needles. Some of these forms are stable; others are unstable, and are sent into the stable forms by vibration.

These experiments can be varied without end. It is certainly interesting to see the mutual effect of two or more vibrating systems, each ruled more or less by the motions of its own superposed magnet; to witness the deformations and decompositions of one molecular arrangement by the vibrations of a neighboring group, to note the changes in form which take place when a larger magnet enters the combination, and to see the deformation of groups produced by the side action of a magnet placed near the bowl.

In the vertical lantern these exhibitions are suggestive of much thought to the student. Of course they are merely suggestions and illustrations of molecular actions and forms; for they exhibit only the results of actions in a plane; so the student should be careful how he draws conclusions from them as to the grouping and mutual actions of molecules in space.

I will here add that I use needles floating vertically and horizontally in water as delicate and mobile indicators of magnetic actions; such as the determination of the position of the poles in magnets, and the displacement of the lines of magnetic force during inductive action on plates of metal, at rest and in motion.

The vibratory motions in the lines of force in the Bell-telephone have been studied from the motions of a needle (floating vertically under the pole of the magnet), caused by moving to and fro through determined distances, the thin iron plate in front of this magnet. These experiments are worth repeating by those who desire clearer conceptions of the manner of action of that remarkable instrument.

ART. XLI.—*On the Intrusive Nature of the Triassic Trap Sheets of New Jersey*; by I. C. RUSSELL.

ALTHOUGH the trap sheets which traverse the Triassic rocks of New Jersey and of the Connecticut Valley are commonly spoken of as being dikes of igneous rocks, yet the proof of their intrusive nature is seldom given. As the igneous origin of these rocks has been questioned by some persons, we take the present opportunity of calling the attention of geologists to a locality where the proof that these sheets of trap were really forced out in a molten condition between the layers of sedimentary rocks, is very positively shown.

The trap ridges of New Jersey have a general north and south direction, usually conformable with the strike of the associated sandstones and shales which compose the great mass of the Triassic formation. The trap rocks, also, seem usually to be conformable in dip with the stratified rocks above and below them. These facts, together with the consideration of the rare occurrence of the exposure of the junction of the trap rocks with the stratified rocks that overlie them—owing to the removal of the latter by denudation, and to the line of contact being covered with drift or overgrown by vegetation—have led to the supposition that the sheets of trap were not intrusive, but were formed cotemporaneously with the shales and sandstones as a bed or stratum of igneous rock, which was spread out in a molten condition at the bottom of the shallow sea in which the stratified rocks were being deposited. The question in hand, then, is to determine (1) whether the plutonic rocks of the Triassic were spread out as a sheet of molten matter and allowed to cool and consolidate before the rocks that rest upon them were deposited, both, therefore, belonging to the same geological period; or (2) were the trap rocks forced out in a fused state among the sedimentary strata after their consolidation, which, consequently, would make them more recent than either the rocks above or below them.

For the purpose of deciding these questions to our own satisfaction, we have examined the trap ridge known as the First Newark Mountain, for some twenty miles of its course in the neighborhood of Plainfield, New Jersey. We hoped by making the following examinations to acquire the desired information in reference to the history of these mountains:

1st. To discover if the sedimentary rocks that repose upon the igneous ones have been changed from their normal condition by the action of heat at the surface of contact.

2d. To determine, if possible, if the trap sheets seem in all cases to be conformable in bedding with the stratified rocks with which they are associated.

It is not difficult to find the junction of these igneous rocks with the shales and sandstones that underlie them. In all such cases that have come under our notice, the stratified rocks have been found to be highly altered and show very plainly that they have been exposed to intense heat. At a number of places beneath the trap rock forming the Palisades on the western shore of the Hudson, this change may be observed; in some instances the sandstone beneath the trap has been metamorphosed into a compact vitreous quartzite. At the Falls of the Passaic the junction of the igneous rocks, with the altered sedimentary rocks beneath, is well exposed. We have also observed similar indications of the action of great heat at a number of places in the shales and sandstones beneath the trap rocks in the neighborhood of Plainfield, New Jersey.

These observations indicate very clearly that the Triassic traps were once in a highly heated and probably molten condition, this is also shown by their crystalline structure. If these fused rocks had been allowed to cool and consolidate, as we were at one time inclined to suppose, before the sandstones and shales above them were deposited, then, of course, the rock resting upon the trap would show no alteration similar to the changes that have affected the stratified rocks beneath.

As we have said, to obtain definite proof of such alteration in the stratified rock above the trap is quite difficult. Although the writer has made himself tolerably familiar with the Triassic formation of New Jersey, and has made many long excursions with the hope of finding such an exposure, yet in only one instance had he been successful. On the western slope of the First Newark Mountain, directly west of Westfield, New Jersey, and near the little deserted village of Feltville, the desired junction is very plainly shown. We there found one page of the history of the Triassic formation clearly legible.

At this locality the stratified rocks are well exposed in the sides of a deep ravine which has been carved out by a small brook that flows down the western slope of the mountain. The stream found but little difficulty in deepening its channel until it came to the hard igneous rocks over which it now flows. The trap rock, which appears in the bed of the stream in some places, presents its usual characteristics of a hard, bluish, crystalline rock, with a conchoidal fracture, similar in every way with the appearance it presents in neighboring localities where it is more thoroughly exposed. In other places it swells up into bosses and rounded masses which are thrust up into the overlying rocks. The outside of these masses present a scoriaceous or slag-like appearance; in the interior the cavities are filled with infiltrated minerals. The shales that rest directly upon these igneous rocks have, in many places, been disturbed from their normal position and greatly altered in texture and color. For the first two or three feet above the trap the shales have been intensely metamorphosed, and are scarcely to be distinguished from the trap itself. In hand specimens it is frequently impossible to determine from their appearance alone which is trap and which is metamorphosed shale. At a distance of six or eight feet above the trap the shales are still very much altered and filled with a great number of small spherical masses of a dark green mineral, resembling epidote. Midway up the sides of the ravine, which is about thirty feet deep, the shales present somewhat of their usual reddish appearance, but are traversed by a great number of irregular cavities formed by the expansion of vapor, while the rocks were in a semi-plastic condition. At a distance of twenty-five or thirty feet above the trap, the shales and sandstones are changed but slightly, if at all, from their normal condition. A bed of limestone from two to three feet in thickness, which is here interstratified with the shales and sandstones—a very rare occurrence in the Triassic formation of New Jersey—where it approaches the trap is considerably altered and forms a mass of semi-crystallized carbonate of lime.

Near the junction of the metamorphosed shales and the igneous rocks beneath, we found in a number of places a peculiar rock composed of angular fragments of a greenish rock, resembling dark-green serpentine in appearance, bound together by a reddish cement—thus forming a typical breccia. This remarkable rock, which we have never seen in any other locality, in some places is two feet or more in thickness, at other times it fills the spaces between concentric masses of igneous rock or metamorphosed shale. Numerous small cavities that occur in the breccia have been filled by infiltration with calcite and zeolites. This interesting material seems to have a history somewhat similar to that of the "friction breccias," mentioned by Von Cotta, as occurring at the margins of eruptive igneous rocks, and formed at the time of their eruption.

The section at Feltville furnishes indisputable evidence that the igneous rocks, composing the First Newark Mountain, were intruded in a molten state between the layers of the stratified rocks subsequent to their consolidation. As these mountains are in every way similar to the rest of the trap ridges which traverse the Triassic regions of New Jersey, from analogy we should extend this conclusion so as to embrace them all.

The distinctness with which this one consideration relating to the Triassic trap sheets has been answered, seems by contrast only to make other questions in their history more obscure. We cannot now determine in what age after the consolidation of the Triassic sedimentary rocks the outbursts of trap occurred. Neither can we decide whether or not the several trap ridges that traverse the Triassic were formed at one time. It may be that one is thousands of years older than its neighbor.

As regards the conformability of the trap sheets with the associated sedimentary rocks, we have but little information to offer, independent of the section at Feltville which we have already described. The curved course which a number of the trap ridges in New Jersey follow, seems to indicate that they must cut across the strata of the sedimentary rocks, which, throughout the whole Triassic area in New Jersey, have a nearly uniform dip of from twelve degrees to fifteen degrees toward the northwest.

ART. XLII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND,* Professor of Physics in the Johns Hopkins University, Baltimore, Md.

Preliminary Remarks.

SINCE the classical determination of the absolute unit of electrical resistance by the Committee on Electrical Standards of the British Association, two re-determinations have been made, one in Germany and the other in Denmark, which each differ two per cent from the British Association determination, the one on one side and the other on the other side, making a total difference of four per cent between the two. Such a great difference in experiments which are capable of considerable exactness, seems so strange that I decided to make a new determination by a method different from any yet used, and which seemed capable of the greatest exactness; and to guard against all error, it was decided to determine all the important factors in at least two different ways, and to eliminate most of the corrections by the method of experiment, rather than by calculation. The method of experiment depended upon the induction of a current on a closed circuit, and in this respect resembled that of Kirchhoff, but it differed from his inasmuch as, in my experiment, the induction current was produced by reversing the main current, and in Kirchhoff's by removing the circuits to a distance from each other. And it seems to me that this method is capable of greater exactness than any other, and it certainly possessed the greatest simplicity in theory and facility in experiment.

In the carrying out of the experiment I have partly availed myself of my own instruments and have partly drawn on the collection of the University, which possesses many unique and accurate instruments for electric and magnetic measurements. To insure uniformity and accuracy, the coils of all these instruments have been wound with my own hands and the measurements reduced to a standard rule which was again compared with the standard at Washington. Unlike many German instruments, quite fine wire has always been used and the number of coils multiplied, for in this way the constants of the coils can be more exactly determined, there is less relative action from the wire connecting the coils, and above all we know exactly where the current passes.

The experiment was performed in the back room of a small house near the University, which was reasonably free from magnetic and other physical disturbances. As the magnetic disturbance was eliminated in the experiment, it was not necessary to select a region entirely free from such disturbance. The small probable error proves that sufficient precaution was taken in this respect.

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* I am greatly indebted to Mr. Jacques, Fellow of the University, who is an excellent observer, for his assistance during the experiment, particularly in reading the tangent galvanometer.

The result of the experiment that the British Association unit is too great by about ·88 per cent, agrees well with Joule's experiment on the heat generated in a wire by a current, and makes the mechanical equivalent as thus obtained very nearly that which he found from friction: it is intermediate between the result of Lorenz and the British Association Committee; and it agrees almost exactly with the British Association Committee's experiments, if we accept the correction which I have applied below.

The difference of nearly three per cent which remains between my result and that of Kohlrausch is difficult to explain, but it is thought that something has been done in this direction in the criticism of his method and results which are entered into below. My value, when introduced into Thomson's and Maxwell's values of the ratio of the electromagnetic to the electrostatic units of electricity, caused a yet further deviation from its value as given in Maxwell's electromagnetic theory of light: but experiments on this ratio have not yet attained the highest accuracy.

History.

The first determination of the resistance of a wire in absolute measure was made by Kirchhoff* in 1849 in answer to a question propounded by Neumann, in whose theory of electrodynamic induction a constant appeared whose numerical value was unknown until that time. His method, like that of this paper, depended on induction from currents: only one galvanometer was used and the primary current was measured by allowing only a small proportion of it to pass through the galvanometer by means of a shunt, while all the induced current passed through it. But, owing to the heating of the wires, the shunt ratio cannot be relied upon as constant, and hence the defect of the method. At present this experiment has only historical value, seeing that no exact record was kept of it in a standard resistance. However we know that the wire was of copper and the temperature 0° R. and that the result obtained gave the resistance of the wire $\frac{1}{7}$ smaller than Weber found for the same wire at 20° R. in 1851.

In 1851, Weber published† experiments by two methods, first by means of an earth inductor, and second by observing the damping of a swinging needle. Three experiments gave for the resistance of the circuit $1903 \cdot 10^8$, $1898 \cdot 10^8$, and $1900 \cdot 10^8$ $\frac{mm.}{sec.}$, but it is to be noted that a correction of five eighths per cent was made on account of the time, two seconds, which it took to turn the earth-inductor, and that no account was taken of the temperature, although the material was copper. He finds for the value of the Jacobi unit, $598 \cdot 10^7 \frac{mm.}{sec.}$. Three years after that, in 1858, Weber made another determination of the specific resistance of copper.* But these determinations were more to develop the method than for exact measurement, and it was not until 1862† that Weber made an exact determination which he expected to be standard. In this last determination he used a method compounded of his first two methods by which the constant of the galvanometer was eliminated, and the same method has since been used by Kohlrausch in his experiments of 1870. The results of these experiments were embodied in a determination of the value of the Siemens unit and of a standard which was sent by Sir Wm. Thomson. As the old Siemens units seem to vary among themselves one or two per cent, and as the result from Thomson's coil differs more than one per cent from that which would be obtained with any known value of the Siemens unit, we cannot be said to know the exact result of these experiments at the present time. Beside which, it was not until the experiments of Dr. Matthiessen on the electric permanence of metals and alloys, that a suitable material could be selected for the standard resistance.

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* **Bestimmung der Constanten von welcher die Intensität inducirter elektrischer Ströme abhängt. Pogg. Ann, Bd. 76, S. 412.**

† **Elektrodynamische Maasbestimmungen; or Pogg. Ann., Bd. 82, S. 337.**

The matter was in this state when a committee was appointed by the British Association in 1861, who, by their experiments which have extended through eight years, have done so much for the absolute system of electrical measurements. But the actual determination of the unit was made in 1863–4. The method used was that of the revolving coil of Sir William Thomson, the principal advantage of which was its simplicity and the fact that the local variation of the earth's magnetism was entirely eliminated and only entered into the calculation as a small correction. The principle of the method is of extreme beauty, seeing that the same earth's magnetism which causes the needle at the center of the coil to point in the magnetic meridian also causes the current in the revolving coil which deflects the needle from that meridian. Whenever a conducting body moves in a magnetic field, currents are generated in it in such direction that the total resultant action is such that the lines of force are apparently dragged after the body as though they met with resistance in passing through it: and so we may regard Thomson's method as a means of measuring the amount of this dragging action.

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* Abh. d. Kön. Ges. d. Wissenschaften zu Göttingen. Bd. 5.

† Zur Galvanometrie; Göttingen, 1862. Also Abh. d. K. Ges. d. Wis. zu Göttingen, Bd. 10.

But, however beautiful and apparently simple the method may appear in theory, yet when we come to the details we find many reasons for not expecting the finest results from it. Nearly all these reasons have been stated by Kohlrausch, and I can do barely more in this direction than review his objections, point out the direction in which each would affect the result, and perhaps in some cases estimate the amount.

In the first place, as the needle also induced currents in the coil which tended in turn to deflect the needle, the needle must have a very small magnetic moment in order that this term may be small enough to be treated as a correction. For this reason the magnetic needle was a small steel sphere 8 mm. diameter, and not magnetized to saturation. It is evident that in a quiescent magnetic field such a magnet would give the direction of the lines of force as accurately as the large magnets of Gauss and Weber, weighing many pounds. But the magnetic force due to the revolving coil is intermittent and the needle must show as it were the average force, together with the action due to induced magnetization. Whether the magnet shows the average force acting on it or not, depends upon the constancy of the magnetic axis, and there seems to be no reason to suppose that this would change in the slightest, though it would have been better to have made the form of the magnet such that it would have been impossible. The induced magnetism of the sphere would not affect the result, were it not for the time taken in magnetization: on this account the needle is dragged with the coil, and hence makes the deflection greater than it should be, and the absolute value of the Ohm too small by a very small quantity. The currents induced in the suspended parts also act in the same direction. Neither of these can be estimated, but they are evidently very minute.

The mere fact that this small magnet was attached to a comparatively large mirror which was exposed to air currents could hardly have affected the result, seeing that the disturbances would have been all eliminated except those due to air currents from the revolving coil, and which we are assured did not exist from the fact that no deflection took place when the coil was revolved with the circuit broken. In revolving the coil in opposite directions very different results were obtained, and the explanation of this has caused considerable discussion. As this is of fundamental importance I shall consider it in detail.

The magnet was suspended by a single fiber seven feet long, and the deflection was diminished by its torsion ·00132. No mention is made of the method used for untwisting the fiber, and we see that it would require only 2·11 turns to deflect the needle 1° from the meridian. To estimate the approximate effect of this, we may omit from Maxwell's equation* all the other minor corrections and we have

$$R=\tfrac{1}{2}\frac{GKw\cos\varphi}{\sin\varphi+t(\varphi-\beta)}=\tfrac{1}{2}\frac{GKw}{\tan\varphi(1+t)}\frac{1}{\left(1-\dfrac{\beta t}{\sin\varphi(1+t)}\right)}\text{ nearly,}$$

where we have substituted $\varphi-\beta$ for φ in Maxwell's equation in the term involving t. In this equation φ is measured from the magnetic meridian; but let us take ψ as the angle from the point of equilibrium. Then $\psi'=\varphi'+\alpha$ and $\psi''=\varphi''-\alpha$, where ψ' and φ' are for negative rotation and ψ'' and φ'' for positive rotation and $\alpha=\text{arc}\sin\dfrac{\beta t}{1+t}$.

Let $$C=\frac{2}{GKw}.$$

Then $$CR'=\frac{1}{\tan\psi'(1+t)}.$$

$$CR''=\frac{1}{\tan\psi''(1+t)}.$$

$$R_{,}=\tfrac{1}{2}(R'+R'').$$

Where R′ and R″ are the apparent values of the resistance as calculated from the negative and positive rotations, and $R_{,}$ is the mean of the two as taken from the table published by the British Association Committee. If R is the true resistance,

$$CR=\frac{1}{\tan\varphi'(1+t)\left(1+\dfrac{\sin\alpha}{\sin\varphi'}\right)}=\frac{1}{\tan\varphi''(1+t)\left(1-\dfrac{\sin\alpha}{\sin\varphi''}\right)}.$$

We shall then find approximately

$$R=\frac{1+\tan\psi'\tan\alpha}{R'\left(1+\dfrac{\sin\alpha}{\sin\varphi'}\right)\left(1-\dfrac{\tan\alpha}{\tan\psi'}\right)}=\frac{1-\tan\psi''\tan\alpha}{R''\left(1-\dfrac{\sin\alpha}{\sin\varphi''}\right)\left(1+\dfrac{\tan\alpha}{\tan\psi''}\right)}.$$

When α is small compared with ψ'' or ψ', and when these are also small, we have

$$R=R_{,}(1+\alpha^2(\alpha^2-\tfrac{1}{2}\psi^2)+\&c.).$$

So that by taking the mean of positive and negative rotations, the effect of torsion is almost entirely eliminated. Now α is the angle by which the needle is deflected from the magnetic meridian by the torsion and its value is $\dfrac{1}{2\psi}\left(1-\dfrac{R'}{R''}\right)$ nearly, when α is small, and this, in one or two of their experiments, exceeds unity or α exceeds 28°·6, which is absurd. Taking even one of the ordinary cases where $\frac{R'}{R_{\prime}}=1{\cdot}02$ and ψ is about $\frac{1}{30}$, we have $\alpha=12°$· nearly, which is a value so large that it would surely have been noticed. Hence we may conclude that no reasonable amount of torsion in the silk fiber could have produced the difference in the results from positive and negative rotation, as has been stated by Mr. Fleming Jenkin in his "Report on the New Unit of Electrical Resistance."*

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* "Reports on Electrical Standards," p. 103.

The greatest value which we can possibly assign to α which might have remained unnoticed is $\frac{1}{18}$, which would not have affected the experiment to any appreciable extent.

Another source of error which may produce the difference we are discussing is connected with the heavy metal frame of the apparatus, in which currents can be induced by the revolving coil. The coil passes so near the frame-work that the currents in it must be quite strong and produce considerable magnetic effect. Kohlrausch has pointed out the existence of these currents, but has failed to consider the theory of them. Now, from the fact that after any number of revolutions the number of lines of force passing through any part of the apparatus is the same as before, we immediately deduce the fact that, if Ohm's law be correct, the algebraical sum of the currents at every point in the frame is zero, and hence the average magnetic action on the needle zero. But although these currents can have no direct action, they can still act by modifying the current in the coil; for while the coil is nearing one of the supports the current in the coil is less than the normal amount, and while it is leaving it it is greater; and although the total current in the coil is the normal amount, yet it acts on the needle at a different angle. By changing the direction of rotation, the effect is nearly but not quite eliminated. The amount of the effect is evidently dependent upon the velocity of rotation and increases with it in some unknown proportion, and the residual effect is evidently in the direction of making the action on the needle too small and thus of increasing R. If these currents are the cause of the different values of R obtained with positive and negative rotation, we should find that, if we picked out those experiments in which this difference was the greatest, they should give a larger value of R than the others. Taking the mean of all the results† in which this difference is greater than one per cent, we find for the Ohm $1{\cdot}0033\,\frac{\text{earth quad.}}{\text{sec.}}$, and when it is less than one per cent, $\cdot9966\,\frac{\text{earth quad.}}{\text{sec.}}$, which is in accordance with the theory, the average velocities being $\frac{100}{28}$ and $\frac{100}{43}$ nearly. But the individual observations have too great a probable error for an exact comparison.

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* "Reports on Electrical Standards," London, 1873, p. 191.

† In the table published by the Committee the different columns do not agree, and I have thought it probable that the last two numbers in the next to the last column should read 1·0032 and 1·0065 instead of 1·0040 and ·9981, and in my discussion I have considered them to read thus.

But whatever the cause of the effect we are considering, the following method of correction must apply. The experiments show that R is a function of the velocity of rotation, and hence, by Taylor's theorem, the true resistance R_0 must be

$$R_0=R(1+Aw+Bw^2+ \&c.),$$

and when R is the mean of results with positive and negative rotations,

$$R_0=R(1+Bw^2+Dw^4+\&c.).$$

Supposing that all the terms can be omitted except the first two, and using the above results for large and small velocities, we find $R_0=\cdot9926\,\frac{\text{earth quad.}}{\text{sec.}}$. But if we reject the two results in which the difference of positive and negative rotations is over seven per cent, we find

$$R_0=\cdot9934\,\frac{\text{earth quad.}}{\text{sec.}}.$$

The rejection of all the higher powers of w renders the correction uncertain, but it at least shows that the Ohm is somewhat smaller than it was meant to be, which agrees with my experiments.

It is to be regretted that the details of these experiments have never been published, and so an exact estimate of their value can never be made. Indeed, we have no data for determining the value of the Ohm from the experiments of 1863. All we know is that, in the final result, the 1864 experiments had five times the weight of those of 1863, and that the two results differed ·16 per cent, but which was the larger is not stated. Now the table of results published in the report of the 1864 experiments contains many errors, some of which we can find out by comparison of the columns. The following corrections seem probable in the eleven experiments: No. 4, second column, read 4·6375 for 4·6275. No. 10, fourth and fifth columns, read 1·0032 and +0·32 in place of 1·0040 and +0·40. No. 11, fourth and fifth columns, read 1·0065 and +0·65 in place of 0·9981 and −0·19. Whether we make these corrections or not the mean value is entirely incompatible with the statement with respect to the 1863 experiments.

With the corrections the mean value of the 1864 experiments is 1 Ohm $= 1{\cdot}00071 \frac{\text{earth quad.}}{\text{sec.}}$, and without them, using the fourth column, it is 1·00014. With the corrections the difference between fast and slow rotation is ·6 per cent.

In the year 1870 Professor F. Kohlrausch made a new determination of Siemen's unit in absolute measure, the method being one formed out of a combination of Weber's two methods of the earth inductor and of damping, by which the constant of the galvanometer was eliminated, and is the same as Weber used in his experiments of 1862. His formula for the resistance of the circuit, omitting small corrections, is

$$w = \frac{32S^2T^2t_0(\lambda - \lambda_0)AB}{\pi^2K \quad (A^2+B^2)^2} \text{ approximately,}$$

where S is the surface of the earth inductor, T is the horizontal intensity of the earth's magnetism, K the moment of inertia of the magnet, t_0 the time of vibration of the magnet, λ the logarithmic decrement, and A and B are the arcs in the method of recoil.

One of the principal criticisms I have to offer with respect to this method is the great number of quantities difficult to observe, which enter the equation as squares, cubes, or even fourth powers. Thus S^2 depends upon the fourth power of the radius of the earth inductor. Now this earth inductor was wound years before by W. Weber, and the mean radius determined from the length of wire and controlled by measuring the circumference of the layers. Now the wire was nearly 3·2 mm. diameter with its coating, and the outer and inner radii were 115· mm. and 142 mm. Hence the diameter of the wire occupied two per cent of the radius of the coil, making it uncertain to what point the radius should be measured. As the coil is wound, each winding sinks into the space between the two wires beneath, except at one spot where it must pass over the tops of the lower wires. The wire must also be wound in a helix. All these facts tend to diminish S and make its value as deduced from the length of the wire too large; and any kinks or irregularities in the wire tend in the same direction. And these errors must be large in an earth-inductor of such dimensions, where the wire is so large and many layers are piled on each other. If we admit an error of one-half a millimeter in the radius as determined in this way, it would diminish the value of S^2 1·4 per cent, and make Kohlrausch's result only ·6 per cent greater than the result of the British Association Committee.

Three other quantities, T, λ and K, are very hard to determine with accuracy, and yet T enters as a square. It is to be noted that this earth-inductor is the same as that used by Weber in his experiment of 1862, and which also gave a larger value to the Ohm than those of the British Association Committee. *Indeed, the results with this inductor and by this method form the only cases where the absolute resistance of the Ohm has been found greater than that from the experiments of the British Association Committee.*

There seems to be a small one-sided error in A and B which Kohlrausch does not mention, but which Weber, in his old experiments of 1851, considered worthy of a ·6 per cent correction, and which would diminish $\frac{AB}{(A^2+B^2)^2}$ by 1·2 per cent. This is the error due to loss of time in turning the earth-inductor. As Kohlrausch's needle had a longer time of vibration than Weber's, the correction will be much smaller. In Weber's estimate the damping was not taken into account, and indeed it is impossible to do so with exactness. To get some idea of the value of the correction, however, we can assume that the current from the earth-inductor is uniform through a time t''', and the complete solution then depends on the elimination of nine quantities from ten complicated equations, and which can only be accomplished approximately. If γ is the true value of the angular velocity, as given to the needle by the earth-inductor, and γ is the velocity as deduced from the ordinary equation for the method of recoil, I find

$$\frac{\gamma}{\gamma_{\prime}}=1+\tfrac{1}{24}\left(\pi\frac{t}{T}\right)^2\frac{\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)(1+\varepsilon^4\lambda)}{(1+\varepsilon^2\lambda)^2}+\text{\&c.},$$

where λ is the logarithmic decrement, ε the base of the natural system of logarithms, T the time of vibration of the needle, and t the time during which the uniform current from the earth-inductor flows. In the actual case, the current from the earth-inductor is nearly proportional to sin t, and hence it will be more exact to substitute

$$4\left(\frac{t}{\pi}\right)^2\int_0^{\frac{1}{2}\pi} t\sin t\,dt=4\left(\frac{t}{\pi}\right)^2$$

in the place of t^2. The formula then becomes

$$\frac{\gamma}{\gamma_{\prime}}=1+\tfrac{1}{6}\left(\frac{t}{T}\right)^2\left(1-9\left(\frac{\lambda}{\pi}\right)^2\right)\frac{1+\varepsilon^4\lambda}{(1+\varepsilon^2\lambda)^2}+\text{\&c.}$$

This modification is more exact when λ is small than when it is large, but it is sufficiently exact in all cases to give some idea of the magnitude of the error to be feared from this source. Kohlrausch does not state how long it took him to turn his earth-inductor, but as T=34 seconds, we shall assume

$\frac{t}{T}=\frac{1}{10}$, and as $\lambda=\frac{1}{2}$ nearly, we have

$$\frac{\gamma}{\gamma_{,}}=1·0008,$$

which would diminish the value of the resistance by ·16 per cent.

As the time we have allowed for turning the earth-inductor is probably greater than it actually was, the actual correction will be less than this.

The correction for the extra current induced in the inductor and galvanometer, as given by Maxwell's equation,* has been shown by Stoletow to be too small to affect the result appreciably.

We may sum up our criticism of this experiment in a few words. The method is defective because, although absolute resistance has the dimensions of $\frac{\text{space}}{\text{time}}$, yet in this method the fourth power of space and the square of time enter, besides other quantities which are difficult to determine. The instruments are defective, because the earth-inductor was of such poor proportion and made of such large wire that its average radius was difficult to determine, and was undoubtedly over-estimated.

It seems probable that a paper scale, which expands and contracts with the weather, was used. And lastly, the results with this inductor and by this method have twice given greater results than anybody else has ever found, and greater than the known values of the mechanical equivalent of heat would indicate.

The latest experiments on resistance have been made by Lorenz of Copenhagen,† by a new method of his own, or rather by an application of an experiment of Faraday's. It consists in measuring the difference of potential between the center and edge of a disc in rapid rotation in a field of known magnetic intensity.

A lengthy criticism of this experiment is not needed, seeing that it was made more to illustrate the method than to give a new value to the Ohm. The quantity primarily determined by the experiment was the absolute resistance of mercury, and the Ohm will have various values according to the different values which we assume for the resistance of mercury in Ohms.

One of the principal defects of the experiment is the large ratio between the radius of the revolving disc and the coil in which it revolved.

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* "Electricity and Magnetism," Art. 762.

† Pogg. Ann., Bd. cxlix, (1873), p. 251.

In conclusion I give the following table of results, reduced as nearly as possible to the absolute value of the Ohm in $\frac{\text{earth quad.}}{\text{sec.}}$*

Date.	Observer.	Value of Ohm.	Remarks.
1849	Kirchhoff	·88 to ·90	Approximately.
1851	Weber	·95 to ·97	"
1862	Weber	1·088	From Thomson's unit.
		1·075	From Weber's value of Siemen's unit.
1863–4	B. A. Committee	1·0000	Mean of all results.
		·993	Corrected to a zero velocity of coil.
1870	Kohlrausch	1·0196	
1873	Lorenz	·970	Taking ratio of quicksilver unit to Ohm =·962.
		·980	Taking ratio of quicksilver unit to Ohm =·953.
1876	Rowland	·9912	From a preliminary comparison with the B. A. unit.

[To be continued.]

ART. XLIII.—*On Croll's Hypothesis of the Origin of Solar and Sidereal Heat;* by Professor DANIEL KIRKWOOD.

THE Quarterly Journal of Science for July, 1877, contains an able and interesting article by James Croll, LL.D., F.R.S., on the age and origin of the sun's heat. The theory of Dr. Croll may be regarded as a compromise between the mathematicians, represented by Thomson, Tait and Newcomb, and the geologists of the uniformitarian school, represented by Playfair, Lyell, Darwin, etc. The principal points of this remarkable paper are:

1. That, as had been estimated by Sir William Thomson and others, but twenty million years' heat could be produced by the falling together of the sun's mass.

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* Since this was written, a new determination has been made by H. F. Weber of Zurich, in which the different results agree with great accuracy. The result has been expressed in Siemen's units, and the comparison seems to have been made simply with a set of resistance coils and not with standards. The modern Siemen's units seem to be reasonably exact, but from the table published by the British Association Committee in 1864, it seems that at that time there was uncertainty as to its value. He obtains 1 S. U. = ·9550 $\frac{\text{earth quad.}}{\text{sec.}}$, which is greater or less than the British Association determination, according as we take the different ratios of the Siemens to the British Association unit, ranging from ·14 per cent above to 1·92 per cent below. In any case the result agrees reasonably well with my own. The apparatus used does not seem to have been of the best, and the exact details are not given. But wooden coils to wind the wire on seem to have been used, which should immediately condemn the experiment where a pair of coils is used, seeing that in that case the constant, both of magnetic effect and of induction, depend on the distance of the coils. It is unfortunate that sufficient details are not given for me to enter into a criticism of the experiment.

2. That not less than five hundred millions of years have been required for the stratification of the earth's crust at the present rate of subaërial denudation; and hence that the gravitation theory of the origin of the sun's heat is incompatible with geological facts.

3. If we suppose two solid opaque bodies, each equal to half the sun's mass, to fall together in consequence simply of their mutual attraction, the collision would instantly generate sufficient heat to reduce the entire mass to a state of vapor. If, in addition to the motion resulting from their mutual attraction, we suppose the bodies to have had an original or independent motion towards each other of 202 miles per second, the concussion would produce 50,000,000 years' heat: a motion of 678 miles per second, together with that due to their mutual attraction, would generate 200,000,000 years' heat; and a velocity of 1,700 miles per second would generate an amount of heat which would keep up the supply at the present rate for 800,000,000 years.

4. The sun and all visible stars may have derived their heat from the collision of cold, opaque masses thus moving in space. The nebulæ are the products of the more recent impacts, and the stars have been formed by the condensation of ancient nebulæ.

5. This theory, while accepting the doctrine of the conservation of energy, indicates at the same time a possible supply of heat for several hundred millions of years; thus satisfying all moderate demands for geological time.

The mathematical correctness of the theory here stated will not be called in question. We shall consider merely the *probability* of the facts assumed as its basis. To the present writer the hypothesis seems unsatisfactory for the following reasons:

1. The existence of such sidereal bodies as the theory assumes is purely conjectural, unless it be claimed that lost or missing stars have become non-luminous, of which we have no conclusive evidence.

2. But, granting their existence, we have no instances of stellar motion comparable with those demanded by the hypothesis; the velocity being in most cases less than 50 miles, and in no case exceeding 200 miles per second.

3. If the two masses by whose collision the sun is supposed to have been formed were very unequal, as would be most probable, the amount of heat generated would be correspondingly less.

4. Such collisions as the theory assumes are wholly hypothetical. It is infinitely improbable that two cosmical bodies should move in the same straight line; and of two moving in different lines, it is improbable that either should impinge

against the other. Comets pass *round* the sun without collision against it.

5. But granting these difficulties removed, let us suppose that about 800,000,000 years ago two cold, opaque bodies, each containing one-half the matter of the solar system, were approaching one another in the same straight line, each at the rate of 1,700 miles per second;* that on meeting, their motion was transformed into heat; and that their united mass was at once reduced to vapor: the great question yet remains—How much of the period represented by these 800,000,000 years' heat can be claimed as *geological* time? The nebula formed by the collision would extend far beyond the present orbit of Neptune. The amount of heat radiated in a given time from so vast a surface would doubtless be much greater than that now emitted in an equal period. No considerable contraction could occur until a large proportion of the heat produced by the impact had been dissipated in space. It has been shown by Trowbridge† that with a temperature at the sun's surface of twice its present intensity the solar atmosphere would be expanded beyond the earth's orbit. The conclusion seems inevitable that much the greater part of the 800,000,000 years' heat must have been radiated into space before the planets were separated from the solar mass, and consequently, that the amount of geological time cannot, to any great extent, have exceeded the limits indicated by the researches of Sir William Thomson.

Upon the whole, it seems more difficult to grant the demands of Dr. Croll's hypothesis than to believe that in former ages the stratification of the earth's crust proceeded more rapidly than at present. The former, as we have seen, has no sufficient basis in the facts of observation. On the other hand, if our planet has cooled down from a state of igneous fluidity, the great heat of former times must doubtless have intensified both aqueous and atmospheric agencies in producing modifications of the earth's exterior.

Bloomington, Indiana, March, 1878.

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* This is the greatest velocity mentioned by Dr. Croll. An increased rate of motion would, of course, produce more heat, but the hypothesis would be open to the same objections.

† Proc. Am. Phil. Soc., vol. xvi, pp. 329-333 and National Quarterly Review, March, 1877, p. 292.

ART. XLIV.—*On the chemical composition of Guanajuatite, or Selenide of Bismuth, from Guanajuato, Mexico*; by J. W. MALLET, University of Virginia.

THIS mineral seems to have been first noticed by Señor Castillo in March, 1873, and was by him partially described[*] as a sulpho-selenide of bismuth.

In the Guanajuato journal "La Republica" for July 13, 1873, Fernandez[†] published a full description, giving to the mineral the name Guanajuatite, and stating that it is solely a selenide of bismuth, a small amount of sulphur found being attributed to admixture with a little pyrite. In the same year or 1874 Rammelsberg[‡] obtained as the result of a partial examination on a very small quantity,

Selenium	16·7
Bismuth	65·4
	82·1

and suggested the presence of zinc. The mineral was more fully examined by Frenzel,[§] whose analysis yielded,

Selenium	24·13
Sulphur	6·60
Bismuth	67·38
	98·11

whence the formula has been deduced—$2Bi_2Se_3 . Bi_2S_3$.

In the 2d Appendix to the 5th edition of Dana's Mineralogy[‖] the name Frenzelite was proposed for the species, but this has subsequently been retracted[¶] in favor of the prior claim of the name Guanajuatite given by Fernandez.

The above are up to this time, I believe, the only published notices of the mineral in question. They leave two doubts in regard to its composition, namely, whether sulphur is really a constituent or only found from accidental admixture, and whether zinc is present or not.

At the Philadelphia Exhibition of 1876, my friend Señor Mariano Bárcena, of the Mexican Commission, was kind enough to give me authentic specimens of this mineral, partly in the original condition as found, and partly reduced to powder. I have availed myself of the opportunity thus afforded of attempting to settle the above questions by careful repetition of the chemical analysis. The already pulverized specimen was chiefly used, but was supplemented by a portion of the other —neither was altogether free from the hydrous silicate of aluminum which constitutes the gangue.

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[*] Naturaleza, ii, 174 (1873); Jahrb. Min. (1874), 225.

[†] Quoted in this Journal, April, 1877, p. 319.

[‡] 2d App. to 5th ed. Dana's Mineralogy (March, 1875), p. 22.

[§] Jahrb. Min. (1874), 679.

[‖] Loc. cit.

[¶] This Journal, loc. cit.

The method employed was the following. Water having been driven off by careful heating in a slow stream of carbon dioxide gas, collected and weighed, the mineral was mixed with ten times its weight of potassium cyanide and fused in an atmosphere of hydrogen. The mass on cooling was treated with water, and the solution filtered; the residue on the filter dried and again fused with the cyanide to ensure complete decomposition, repeating the treatment with water and filtration. From the mixed filtrates selenium was thrown down by addition of hydrochloric acid in excess, filtered after thirty-six hours on a weighed filter, cautiously dried and weighed; it was then burned, and a minute amount of silica left behind was determined. The solution from which the selenium had been precipitated was treated with potassium per-manganate as long as decolorization took place, and barium chloride then added; from the weight of barium sulphate thrown down sulphur was determined. The remaining solution was then evaporated to dryness at 100° C., the residue moistened with hydrochloric acid and treated with boiling water, leaving a further trace of silica; manganese (from the per-manganate used) and aluminum were now precipitated by ammonium sulphide, and separated by barium carbonate, the alumina being determined. The original residue of bismuth, left on the filter when the selenio-cyanate of potassium was filtered off, was dissolved in nitric acid, evaporated to dryness to separate a further portion of silica, redissolved, the bismuth thrown down by hydrosulphuric acid, filtered off, and a further portion of alumina (with a trace of ferric oxide) recovered from the filtrate. Lastly, the bismuth sulphide was carefully reduced by fusion with potassium cyanide, and weighed as metal.

The results were,

Selenium	31·64
Sulphur	·61
Bismuth	59·92
Alumina	2·53
Ferric oxide	*trace*
Silica	3·47
Water	1·46
	99·63

Zinc was specially looked for, both in the general analysis and using a separate portion for this purpose alone, but none could be found. Possibly, as Rammelsberg had but a very small quantity of material on which to work, he may have been led to suspect the presence of zinc by a precipitate of aluminum hydrate derived from gangue.

No evidence, physical or chemical, could be found of the presence of pyrite; the trace (unweighable) of iron appears to belong to the gangue.

It is stated that this gangue is galapectite (Halloysite); if the amount of such mineral present be calculated from the alumina the above figures represent the specimen as composed of—

Guanajuatite		92·17
Gangue	Halloysite	6·72
	Quartz	·56
Moisture		·18
		99·63

and the Guanajuatite in the pure state would consist of

Selenium	34·33
Sulphur	·66
Bismuth	65·01
	100·00

Hence we have the atomic ratio,

$$Bi : Se : S = 310 : 432 : 21,$$

or, uniting the sulphur with selenium,

$$Bi : Se = 310 : 453 = 2{\cdot}000 : 2{\cdot}922,$$

a close approximation to 2 : 3, justifying of course the formula Bi_2Se_3.

The quantity of sulphur present is too small to warrant the assumption that it bears a simple atomic proportion to the selenium, but the former element certainly is present, and not as pyrite. One can scarcely suppose that in Frenzel's analysis nearly six per cent of iron was overlooked, as it must have been if pyrite were the cause (as suggested by Fernandez) of the occurrence of the sulphur found.

It seems clear that the mineral in question must be viewed as sesqui-selenide of bismuth, with isomorphous replacement to a variable extent of selenium by sulphur.

It is also mentioned (this Journal, April, 1867) that Fernandez has described a second selenide of bismuth from the same locality, and has derived from his analyses of more or less pure specimens the formula Bi_3Se. This formula is very improbable, since it involves the presence of an odd number of perissad atoms. Perhaps there may have been an admixture of native (metallic) bismuth.

ART. XLV.—*On the Janssen Solar Photograph and Optical Studies;* by S. P. LANGLEY.

MR. JANSSEN, in papers lately presented to the French Institute[*] has given an account of recent results in solar-photography, obtained by him at the observatory of Meudon, and from the comments of Messrs. Huggins, Lockyer, De la Rue and other competent judges, it has been understood that remarkable advances have been made over any before produced. A copy has been published in the Annuaire du Bureau des Longitudes, but details cannot well be studied from such a print, on paper, and details are here all-important. As the writer has received, through the courtesy of M. Janssen, a fine positive on glass; the only one he knows of, as having yet reached this country, and as it not only bears important testimony to facts which have rested hitherto in part on statements in previous numbers of this Journal, but is in every way so remarkable as to constitute an important step in the history of solar physics, an account will be given of it here.

The means used by M. Janssen for producing the photograph are understood to be a telescope of about five or six inches aperture, and twelve feet focal length, embodying the improvements introduced by Mr. Rutherfurd, and others of M. Janssen's own, the most important change being a shortening the time of exposure to $\frac{1}{3000}$ of a second, and at the same time an enlargement of the image. This involves a modification of the time of development, etc., and has evidently cost study and labor, the arduousness of which may be inferred from the perfection of the result.

As the photograph can only be made generally intelligible by some sort of illustration, the reader is referred to two papers in this Journal, the first in the number for February, 1874, where an article on the minute structure of the photosphere is illustrated by an Albertype plate, the second in March, 1875, accompanied by a steel engraving. The reader must also be detained briefly over a question of nomenclature which possibly has caused a misapprehension on Mr. Janssen's part, for the first paper undertook among other things, to specifically show that the elements of the photosphere were *not* "willow-leaf-" or "rice-grain-" like forms as had been commonly supposed. The term "rice-grain," it was carefully explained, was incorrect, and as an illustration imperfect. The name "rice-grain," for want of a generally accepted word, was used in the article, but under protest; especially as I had myself resolved these into minuter components, and carefully drawn specific instances of the foliate form and subdivision, specially calling attention to them in the plate, where they are found in two squares surrounded by a heavy outline. It is necessary to insist on the fact that purely optical methods had informed us of the nature of the constituents of the surface with a minuteness which photography has not even now attained. It was also stated in the first of these articles that the estimated mean distances between the centers of these composite objects ranged from 2″·57 to 1″·42 according to the degree of disintegration introduced by magnifying power, and the very important conclusion was reached that the light of the sun comes to us chiefly from an extremely small part of its surface—an indefinitely small part, but which is at any rate *less than one-fifth* of the whole. M. Janssen's impression that the true form and relative area of these has first been shown by the photograph is a misapprehension, though arising most naturally in part from the vicious nomenclature of the subject.

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[*] Comptes Rendus, Oct. 29th and Dec. 31st, 1877.

Until lately, photography has been useful, chiefly in fixing the positions and sizes of spots on the sun, rather than in studies of detail, which have hitherto been carried on with success only by the eye. The remarkable photograph before us begins a new order of things, for, though as we have said it does not reach absolutely all that the eye has yet caught, yet only those who have watched the sun with powerful instruments for years, can have enjoyed (and that rarely) the opportunity of seeing more than is here fixed for leisure study. It need hardly be said how immense is the gain of this opportunity for all to examine and verify deliberately; and it should be stated further that the photograph not only confirms previous results which have rested on the testimony of one or two individuals, but adds at least one important one of its own.

The plate before us is, as has been said, a positive, the solar image being 303^{mm}, or nearly twelve inches in diameter. The image viewed at a distance, shows the usual darkening toward the edge, though not quite uniformly all round, a circumstance probably not in the present case significant of any solar irregularity. Upon a closer view we see the coarse vague maculations or marblings* (formed as it seems to me by waves in the solar atmosphere, causing regions of greater thickness and consequently greater absorption by the heaping of the "rice grains"† and in some degree by their unequal distribution).

On a close approach we see the granular structure of the photosphere as it has never before been rendered in photography.

To give an idea of the precision of the photograph without the plate is hardly possible, but as the individual "grains" may be here counted, I have placed on the positive a Rogers' reticule, consisting of very small squares, engine-divided on glass, which had been actually used on the sun for a similar purpose; and with its aid counted the "grains" in different parts of the plate not far from the center of the image, and taking the average I find a mean of about 5200 "grains" to the square inch, whence it appears, that at this rate, the plate actually exhibits within its circumference over half a million of these objects. From this measurement and the solar angular semi-diameter, it also appears that their mean distance in centers is 2″·2 which is in close agreement with optical determinations made with corresponding powers. As these objects are rather complex than simple, I venture the opinion that should M. Janssen succeed in future in enlarging his photograph while retaining his present wonderful definition, that their mean distance will tend to appear still less.

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* This is seen in the Albertype on removing it five or six yards from the eye where details are lost. The vagueness of the aggregations is in the original (i. e. in the sun itself.)

† Indicated all over the Albertype plates, and shown in specific details in the two designated squares.

We are now brought to what is perhaps the most remarkable feature of the plate, for a continued examination shows what undoubtedly has not been established by optical studies, that there are extensive regions where the "grains" are distinctly seen, and others adjacent where they are confused and blurred as if by bad definition. There are numerous alternations of these areas of disturbance; which are themselves of varying sizes; perhaps it would be more correct to say that the general surface presents this blurry character, with small regions where the definition is as sharp and clear as we have described it. Now,—a question evidently to be asked,—is this bad definition something in the solar atmosphere or our own? Does it mean a tremendous disturbance over hundreds of millions of square leagues, or a quivering of the air a few yards from the camera?

First we may ask, how far has there been any anticipation of such local disturbance on the sun, away from the spots and away from the faculæ as seen on the edge? I believe there has been, from telescopic study, a somewhat uncertain recognition that the photospheric structure differed at different times, but nothing like the variations shown here was anticipated. Doubtless these alternations of structure in adjacent regions, if once recognized, would be visible to the telescope, if sought, and it has, in fact, often been asserted that the aspect of the granulations varied at different times from solar causes; but with the telescope we lack the facility for deliberate comparison of one part of the disc with another, we obtain here, since owing to the undulations which we do know without doubt, are in our own atmosphere, our best vision is but momentary, and before we can turn from one part of the sun to compare it with another the opportunity is gone. The photograph, obtained as it is, in less than a thousandth of a second, may be taken many times in succession, and reproduce more or less indefinite images, yet the thousandth exposure may fall in the brief instant of definition the observer patiently watches for, and then the results of this rare moment are made permanent by the camera.

Curiously enough, these disturbances of the solar atmosphere have been more distinctly *felt* by the thermopile, perhaps, than seen by the telescope. In 1874, after summing the results of thermal measurements, carried on at Allegheny to determine the heat of different parts of the surface, and after eliminating the effects of increasing absorption toward the limb, of spots, etc., there remained irregularities of a small order which showed either a hitherto unrecognized solar phenomenon, or (what might conceivably be the case) some undetected causes of minute instrumental error. The difficulty of determining which was great, for it was a characteristic of these minute fluctuations, that they were neither both fixed in position or magnitude, but incessantly, changed place and degree. Only after a year's further study, I felt confident that I had so far eliminated the smaller instrumental errors, that I was in presence of residual phenomena, which, however minute, were real; and which corresponded to continual fluctuations in the depth of the solar atmosphere all over the surface, in the nature of local disturbances caused by its currents, and I felt justified in announcing the existence of these—which I had never seen—in positive terms.*

Nothing could be more unexpected than the confirmation of these statements which the photograph offers, but it would be most unjust to M. Janssen's eminently original and valuable work, not to state that such anticipations, however positive, are obscure compared with the light thrown on this part of the subject by his methods. We say this, under the reserves already hinted, as to the difficulty of distinguishing by a single plate, the exact limits between the effects of solar and telluric disturbance; but so much at least appears to be solar, that we have here something very like evidence of great waves, in the solar atmosphere, obscuring the photospheric structure, and not only obscuring but changing it; for the currents appear to have altered the shapes of the grains, and their disposition. M. Janssen seems to consider the ultimate form of these grains as literal spheres, but it appears to me that his own admirable photograph bears other evidence. The components of these "grains" when undisturbed do indeed tend to dome-like forms, but these have always appeared to me to be but the extremities of filaments; extremities which aggregated, cause the "grains," and which lifted higher than their fellows cause the faculæ; the filaments themselves, being seen a little, here and there on the surface where bent over, and seen at all their length in the spots. In other words, we may compare the pho-

* Comptes Rendus, Sept. 6, 1875, p. 438.

tosphere to a field of grain in which from a bird's-eye view, we see, in a calm, only the rounded summits of the wheat. Let a wind blow fitfully over the surface, bending the crests here and there and showing more of the form of the straws. This is, it seems to me, the suggested explanation of the elongated form of the "grains" shown in such an interesting manner in M. Janssen's photograph where the action of solar currents is indicated and accompanied with partial obscuration. Let a whirlwind beat down the grain, showing the stalks lying every way exposed throughout their length—these are the filaments in a spot.

Of course the simile is imperfect and is not to be carried further. I can only venture conjecture as to what these "grains" really are, but I have always believed and still believe that they are not mere globular or bubble-like forms, but are associated with something beneath the surface most probably connected with ascending and descending gaseous currents, which in some way bring to that surface the heat from the interior, and carry back to it the gases which have been cooled by radiation, perhaps even to the point where precipitation occurs.

As to the question of the real solar origin of certain of the less definite forms on the plate we may say then, that it is known that certain peculiarities in our own atmosphere tend to impress themselves on the photographic plate, along with the solar phenomena, and it would be doubtless desirable, if possible, that two such photographs as we have here should be taken at intervals of five or ten minutes apart to eliminate this. It has been frequently asked why this is not done. But those who ask such a question are not familiar with the rarity of the instants in which *such* photographs can be taken Two indeed can be taken at any interval, but those like the one before us, demand not only the finest mechanical and chemical methods and still more the highest skill, but atmospheric conditions so brief as to rarely or never last during even the short time mentioned.

Finally, then, though without two photographs of equal excellence taken within a few minutes of each other, it is perhaps impracticable to say *exactly* how much of the inequality of the plate is solar, it seems possible to state from the intrinsic evidence of the plate itself, that on the main features of its most interesting evidence as to the action of solar storms we may rely, and in this statement the writer, distrustful of his own knowledge of photographic processes, has sought the opinion of the most competent judge in these matters before expressing his own.

Our conclusion is, that M. Janssen has accomplished a remarkable, indeed, a wonderful, advance in solar photography, and that his success is not only a brilliant, but a useful one, for which he should have the thanks of every student of solar physics.

Allegheny, Penn., March 14, 1878.

Art. XLVI.—*On the occurrence of a Tree-like fossil plant, Glyptodendron, in the Upper Silurian (Clinton) Rocks of Ohio;* by Professor E. W. Claypole, B.A., B.Sc. (London), of Antioch College, Yellow Springs, Ohio.

In the month of July, 1877, while on a geological excursion in company with one of my students, Mr. Leven Siler, in the vicinity of Eaton, in Preble County, Ohio, the latter picked up and handed me a slab bearing the impression of a vegetable stem, which proved, on closer examination, to be that of a plant allied to Lepidodendron. As the beds in which we were working at the time lie at the very base of the "Clinton" of the Ohio Survey, and within a few feet of the break which marks the summit of the Cincinnati group of the Lower Silurian, the specimen immediately assumed unusual interest and importance; no *indisputable* traces of land-plants having then come to light from so low an horizon in America, and no remains of arborescent vegetation being known *with certainty* from strata of so old a date in the New or Old World.

The slab containing the impression was not taken out of the solid rock, but lay loose on the bank of Clinton limestone. This fact will naturally raise some question concerning its age in the mind of every geologist. Fortunately, however, we are not in this instance dependent upon such evidence. To any one practically familiar with the Clinton rocks as they crop out around the Cincinnati uplift no doubt can arise. The stone is a piece of yellow, rough, encrinital limestone, considerably weathered, with the characteristic appearance of the Clinton at Eaton and here. Moreover, by the side of the impression there lies embedded one of our commonest corals (Polyzoa?) closely resembling *Chætetes lycoperdon* (Hall) of the Clinton in New York, figured in the second volume of the New York Survey. Lastly, from the back of the slab I chipped out a small specimen of an *Illænus*; either *Illænus Daytonensis* (Hall and Whitfield, Ohio Pal., vol. ii, 1875), or *Illænus Barriensis* (Murch. and Hall, 1862) (i. g. *Illænus Ioxus* of Hall, 1847), the mould of which still remains in the slab. Its Silurian age is therefore placed beyond a doubt, no species of Illænus being known in America above the Niagara group.

In describing and naming it, my first thought was to place it in the genus Lepidodendron, as a provisional arrangement pending the discovery of more perfect specimens. But further study of the fossil and its nearest allies among the Sigillarids and Lepidodendrids has induced me to place it by itself in a new genus, which seems to form a connecting link between some other paleozoic genera. I append the following description:—

GLYPTODENDRON. Tree-like; stem cylindrical; surface marked with two parallel sets of ridges running spirally up the stem in opposite directions, crossing each other and thus forming rhomboidal areoles. Lower portion of areole depressed and probably representing or containing a leaf-scar. Depressed portion of areole (leaf-scar?) symmetrical (i. e. alike on the right and left sides.) Vascular scars, leaves, fruit, etc., unknown. The name is from the Greek γλύφω, I engrave, and alludes to the depressed areoles.

Glyptodendron Eatonense. Stem thick and trunk-like; the specimen from which this description was made measured when complete about six inches in diameter. Surface divided into rhomboidal areoles by two sets of narrow ridges parallel and equidistant, running spirally up the stem in opposite directions. These ridges cross each other nearly at right angles. The areoles thus formed measure about seven-sixteenths of an inch along each diagonal. Lower portion of areole deeply and evenly depressed and probably representing a sunken leaf-scar. Upper border of depressed portion rounded in outline and elevated, equalling in height the spiral ridges. No trace of the vascular scars can be seen in consequence of the roughness of the stone and the weathering it has undergone. Found near Eaton, Preble Co., Ohio.

Being anxious to have the opinion of some naturalist more experienced than myself in the subject of the Pre-carboniferous flora, I sent a drawing of the fossil to Dr. J. W. Dawson of Montreal, one of the best authorities on the subject on this continent. In his reply of Oct. 22, 1877, he expressed his conviction of the importance of the discovery, and, from an inspection of the drawing, suggested its resemblance to *L. tetragonum* St. or *Bergeria* of the Lower Carboniferous, and also to *Diplostegium.* He asked the full extent of the evidence of its age, and expressed a wish to see a cast before forming any opinion upon it. This was sent some time afterward, and in reply, Dr. Dawson has favored me with some notes upon it and with permission to use them here. He says, "The marks on your specimen, at first sight, resemble those of the *Lepidodendra* of the type of the *L. tetragonum* Sternb. (*Bergeria* of some authors), a very widely diffused type of Lower Carboniferous age, found about that horizon in Europe, America and Australia. They may, however, have belonged to a plant of the genus *Ulodendron* or *Lepidophloeus*, and since the vascular scars are not preserved, it is impossible to decide this question. It is further to be observed, that the areoles appear to be deeply depressed, being in this respect the reverse of the leaf-bases of *Lepidodendron.* It may possibly have belonged to a plant of the nature of a Tree-fern, or of a *Sigillaria* allied to *S. Menardi*, rather than to a true *Lepidoden-*

dron." "In speaking of the areoles, I take it for granted that the curvature of the cast represents that of the stem." "The specimen may, however, have been a bit of bark pressed out of shape."

My own opinion, after a careful examination of the original, is that the curvature of the cast *does* represent that of the stem, and consequently that Dr. Dawson's remarks on its resemblances are well founded. The bark of *Lepidodendra*, etc., when pressed as usually occurs in the Coal-measures, is constantly flattened. In a subsequent communication, Dr. Dawson alludes to the possibility suggested above, that the fossil may exhibit a composite character partaking of the character of more than one existing genus. The wide diffusion of the type which it most resembles in the Lower Carboniferous is good reason for believing that it is very ancient, and therefore its occurrence so low as the Clinton limestone is the less surprising.

In conclusion, I gladly express my indebtedness to Dr. J. W. Dawson, of Montreal, for valuable aid cheerfully rendered, and to Mr. Leo Lesquereux, of Columbus, in this State, for prompt and kind replies to letters of enquiry.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On certain Fundamental Thermo-chemical Data.*—**Berthelot** has re-determined with great care the heat data belonging to certain chemical reactions, using an improved calorimeter for the purpose, described in the memoir. The heat of formation of sulphurous oxide, although determined many times, was not up to this time accurately known. Thus sixteen grams of sulphur burned to gaseous sulphurous oxide gave Dulong 41·6 calories, Hess 41·1, Andrews 36·9, and Favre and Silbermann 35·6, the discrepancies being accounted for on the supposition that SO_3 was formed. In Berthelot's experiments the calorimeter was of thin glass, and the sulphur used the octahedral variety. The mean result was, for sixteen grams of sulphur, 34·55 calories. From this value, the author calculates the heat of formation of sulphuric acid and the sulphates, $S+O_3=SO_3$ for example (using the old atomic weights) evolving 51·8 calories, $SO+O_2=SO_3$ setting free 17·2, $S+O_3+HO=SO_3HO$ evolving 62·0, $S+O_4+H=SO_4H$, 96·5, $S+O_4+K=SO_4K$ (solid), 171·1, etc. The author also found the heat of combustion of carbonous oxide in his calorimeter to be 68·17 calories, for C_2 or twelve grams of carbon. Hence as C_2 as diamond, in burning to C_2O_4 evolves 94, C_2 in burning to C_2O_2 evolves 94−68·2=25·8, or for amorphous carbon 28·8. Ethylene twenty-eight grams, yields 334·5 calories, acetylene twenty-six

grams, 317·5, and benzene seventy-eight grams, 776 calories. In the formation of the hydrogen compounds of bromine and iodine, H + Br gas = HBr gas yields 13·1 calories, while H + Br liquid = HBr gas gives 9·5, H + I gas = HI gas gives −0·9 calories, and H + I solid = HI gas gives −6·3. The heat of formation of bromic acid is −21·2, and of hypobromous acid −6·7.—*Ann. Chim. Phys.*, V, xiii, 1, Jan. 1878. G. F. B.

2. *Relations between the Atomic Weights of the Elements.*—WAECHTER has described certain additional relations between the atomic weights of certain of the elements and their properties. He formulates his conclusions as follows: 1st. The chemism of the elements tabulated, twenty-nine in number, diminishes from fluorine to silicon, and this with an increasing atomic weight and with an increasing valence; while from silicon to caesium the chemism also increases, the atomic weight also increasing but the valence diminishing. 2d. The arithmetical means of the atomic weights of pairs of elements of equally intense but of opposite attractions (i. e., one positive and the other negative) are nearly equal to one another, being about seventy-six. As for example, F and Cs, N and Di, Te and Mg, O and Ba, I and Na, Sb and Al. 3d. The fusing and the boiling points of the elements, so far as known, increase with increasing atomic weights and increasing valence, from fluorine to silicon; while from silicon to caesium, they diminish as the atomic weight rises and the valence falls. 4th. The specific heats of the elements given in the table, in the solid state, so far as known, diminish with increasing atomic weight and increasing valence. 5th. The specific gravity, in the solid state of the elements as given, is, with corresponding atomic weights, the greater, the higher the valence. 6th. The chemism of the negative metalloids—fluorine to silicon—for the metals proper, diminishes with increasing atomic weight and an increasing valence.—*Ber. Berl. Chem. Ges.*, xi, 11, Jan. 1878. G. F. B.

3. *New Method for the Preparation of Nitrogen.*—GATEHOUSE has observed that when ammonium nitrate and manganese peroxide are heated together up to the fusing point of the former (about 180°) a violent reaction ensues, the mass becomes red hot and nitrogen is evolved. If the temperature be kept between 180° and 200°, the nitrogen is entirely pure, being formed according to the equation:

$$(NH_4NO_3)_4 + MnO_2 = Mn(NO_3)_2 + (H_2O)_8 + N_6.$$

In one experiment, three grams NH_4NO_3 heated with an equal weight of MnO_2 in a mercury bath kept at 205°, yielded 630 cubic centimeters of gas, nearly the theoretical quantity. If the temperature rises above 216° the manganous nitrate decomposes giving nitrous vapors and oxygen. At 221° the gas contained eight per cent of oxygen.—*Bull. Soc. Ch.*, II, xxix, 115, Feb. 1878. G. F. B.

4. *On a Method of separating Crystallized Silica, especially Quartz, from Silicates.*—LAUFER has suggested an improvement

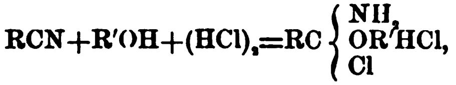
upon Müller's process for separating the silica existing as quartz in rocks and soils from that combined in the admixed silicates, which appears to be satisfactory. It is founded on the well-known property of salt of phosphorus of decomposing silicates, dissolving the metallic oxides and setting the silica free, while at the same time the quartz present is not attacked. The finely pulverized material is weighed in a platinum crucible, sufficient salt of phosphorus to decompose the silicates is added, and the whole is carefully heated, first in an air bath, and finally in the blast till the mass is in quiet fusion. After cooling, the mass is detached, boiled with hydrochloric acid for a long time, filtered, and the residue extracted with boiling solution of soda to remove the silica coming from the silicates. The final residue is washed, dried, and weighed as quartz. On treating a gram of the quartz so obtained with hydrofluoric and sulphuric acids, only a few milligrams of sulphate was left. The errors of the method are only about 0·2 per cent.—*Ber. Berl. Chem. Ges.*, xi, 60, Jan. 1878. G. F. B.

5. *On the use of Stannous Chloride in the analysis of Nitro-compounds.*—LIMPRICHT, noticing the facility with which an acid solution of stannous chloride reduced nitro-compounds, made a series of experiments to ascertain whether the reaction could not be made use of to determine their composition quantitatively, and with excellent success. When a weighed quantity of an organic nitro-compound is heated with a definite volume of stannous chloride solution of known strength, the nitro-group is converted into NH_2 thus: $NO_2+(SnCl_2)_3+(HCl)_6=NH_2+SnCl_4+(H_2O)_2$. By titrition the quantity of stannous chloride used is ascertained; and from this the amount of the NO_2 calculated. The solutions needed are: (1) Stannous chloride, prepared by dissolving 150 grams tin in concentrated hydrochloric acid, decanting, adding 50 c.c. of HCl and diluting to a liter; (2) soda solution, 180 grams anhydrous soda and 240 grams Seignette salt in a liter of water; (3) starch solution diluted and filtered; (4) iodine solution, 127 grams iodine and the necessary quantity of KI, in a liter. Of this tenth-normal solution, 1 c.c. $=$·0059 gr. Sn. $=$0·0007666 gr. NO_2; (5) Permanganate solution, made and titered as usual. In the analysis, 0·2 gram of the nitro-compound is weighed out, placed in a 100 c.c. flask with 10 c.c. of the tin solution, and warmed. After cooling the flask is filled to the mark with water, 10 c.c. is removed with a pipette, placed in a beaker, diluted, mixed with the soda solution until the precipitate at first produced is re-dissolved, and after adding some starch solution, titered with the iodine solution until the blue becomes permanent. The numerous results given show the process to be accurate.—*Ber. Berl. Chem. Ges.*, xi, 35, Jan. 1878. G. F. B.

6. *On a so-called Catalytic action of Carbon Disulphide.*—HELL and MÜHLHÄUSER have observed a remarkable action of carbon disulphide in facilitating the action of bromine upon acetic and formic acids. While bromine and acetic acid, treated in a

water bath in sealed tubes for forty hours, have not the slightest action on each other, the addition of a small quantity of carbon disulphide to the mixture so facilitates the reaction that even in six hours it is completed, and in one case the tube exploded from the evolved hydrogen bromide, in two hours. Further investigation showed that the presence of the disulphide was not absolutely necessary, either for the formation of the addition or the substitution products of bromine and acetic acid, but that it facilitated the formation of both to an extraordinary degree, the time required for the action to be completed being in the exact inverse ratio of the amount of CS_2 present. The precise mode of action of the disulphide, the authors are now engaged in investigating. —*Ber. Berl. Chem. Ges.*, xi, 241, Feb. 1878. G. F. B.

7. *On the Conversion of Nitriles into Amides.*—**Pinner** and **Klein** showed a short time ago that any nitrile may be made to combine directly with any alcohol, by passing gaseous HCl or HBr into a mixture of the two. There is first formed the salt of an amido-chloride:



$$RCN+R'OH+(HCl)_2=RC\left\{\begin{matrix}NH_2\\OR'HCl,\\Cl\end{matrix}\right.$$

which immediately loses HCl and becomes a salt of an imide. Thus benzonitrile and isobutyl alcohol when thus treated give the hydrochlorate of benzimidobutyl ether, $C_6H_5C\left\{\begin{matrix}NH\\OC_4H_9\end{matrix}\right\}HCl$. By the action of alcoholic ammonia, the free benzimidobutyl ether $C_6H_5C\left\{\begin{matrix}NH\\OC_4H_9\end{matrix}\right.$ and benzimido-amide (or benzenylamimide) $C_6H_5C\left\{\begin{matrix}NH\\NH_2\end{matrix}\right\}HCl$ are obtained. The authors now describe benzimidoamide and its silver compound, and the action of ethyl iodide and of acetic oxide upon it, and also benzimidobutyl ether. Also a polymer of benzonitrile termed kyaphenin.—*Ber. Berl. Chem. Ges.*, xi, 4, Jan. 1878. G. F. B.

8. *Quercite a Pentacid Alcohol.*—**Homann** has published in full his memoir on quercite, giving the results of experiments made in Wislicenus's laboratory, which go to prove this sugar to be a pentacid alcohol. By the prolonged action of acetic oxide upon quercite in sealed tubes at 100° to 120°, a penta-acetate was obtained as an amorphous brittle mass, having the formula $C_6H_7(OC_2H_3O)_5$. Saponification with barium hydrate confirmed this composition. The tetra and the diacetate are also described. The pentanitrate or nitroquercite was obtained as a gummy mass, exploding on being heated. Acetyl chloride acts like acetic oxide. Fuming hydrochloric acid was without action.—*Liebig's Ann.*, cxc, 282, Jan. 1878. G. F. B.

9. *On the Acids of Cocoa Butter.*—**Kingzett** has examined the chemical constitution of the Cocoa butter of commerce, having a fusing point of about 30° C. It was saponified by sodium hydrate, the sodium salt converted into a barium salt, this decom-

posed by HCl in presence of ether and the fatty acids obtained by distilling off the ether, were repeatedly crystallized from alcohol. By repeating the fractional crystallizations, there was obtained beside oleic acid, an acid of the formula $C_{12}H_{24}O_2$ and fusing point 57·5° as one extreme, and another $C_{64}H_{128}O_2$, of fusing point 72·2° as the other, both of which are new. To the latter the author gives the name *Theobromic acid.*—*J. Ch. Soc.*, xxxiii, 38, Jan. 1878. G. F. B.

10. *On a new Class of Acid Salts.*—Villiers has studied a new class of acid salts, the acid acetates, in which acetic acid itself plays the part of water of crystallization. Their general formula of the sodium salts is $C_2H_3NaO_2(C_2H_4O_2)\frac{n}{m}H_2O\frac{n'}{m'}$, where $\frac{n}{m}+\frac{n'}{m'}=2$ or 3. Thus, a salt crystallizing in small flat efflorescent prisms has the formula $C_2H_3NaO_2(C_2H_4O_2)\frac{4}{5}(H_2O)\frac{6}{5}$. Another of the second class, where $\frac{n}{m}+\frac{n'}{m'}=3$, is in small efflorescent prisms of the formula $C_2H_3NaO_2(C_2H_4O_2)\frac{1}{4}(H_2O)\frac{11}{4}$.—*Bull. Soc. Ch.*, II, xxix, 153, Feb. 1878. G. F. B.

11. *Electro-Magnetic and Calorimetric Absolute Measurements.*—In the continuation of a paper with the above title, Professor H. F. Weber concludes that the value of the Siemen's mercury unit of resistance lies between $0·9536 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}}\right)$ and $0·9550 \times 10^{10} \left(\frac{\text{millim.}}{\text{sec.}}\right)$ and that the value of the British unit is the value asserted, $10^{10} \left(\frac{\text{millim.}}{\text{sec.}}\right)$. Professor Weber also discusses the experiments of Favre on the quantities of heat developed by various electromotive forces in their circuits during the time in which they consume equal quantities of zinc. Favre asserted that "*the ratio of those quantities of heat gives quite another value than does the ratio of the corresponding electromotive forces when measured galvanometrically,*" and finds that the conclusion is at variance with the galvanic laws of Joule, Ohm and Faraday. Weber from experiment shows that the ratio of the electromotive forces obtained by the heat method and the galvanic method is the same, and attributes the errors of Favre to the use of a mercury calorimeter, an instrument which Weber condemns. —*Phil. Mag.*, March 1878, No. 30, p. 189. J. T.

12. *Chemical Dynamics.*—Dr. C. R. Alder Wright and Mr. A. P. Luff of St. Mary's Hospital, London, have made a first report to the London Chemical Society* of their investigation on this subject. The chief point established by their paper is the general principle that "*that* reducing agent begins to act at the lowest temperature which has the greatest heat of combustion." Details of experiments are given on the reduction of cupric oxide and

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* Journal of Chemical Society of London, January, 1878.

ferric oxide by carbonic oxide, by hydrogen and by carbon respectively; from which it appears that hydrogen acts at a lower temperature than carbon, and carbonic oxide at a lower temperature than hydrogen. The quantities of heat evolved by the combination of carbon, hydrogen, and carbonic oxide with sixteen grams of oxygen are taken as 47·78, 57·82 and 68·35 respectively. The effect of the different physical states both of the metallic oxides and of the charcoal used is likewise carefully discussed.

In connection with the above paper, Mr. M. M. Pattison Muir, of Caius College, Cambridge discusses the "influence exerted by time and mass in certain reactions in which insoluble salts are produced." The author following a previous suggestion of Gladstone has studied chiefly the reactions of potassic and sodic carbonates on calcic chloride in aqueous solution and finds 1, "that the greater portion of the chemical change is produced during the first five minutes of the action; after that time the action very much decreases in rapidity; the amount of change in the next twenty-five or thirty minutes is, however, always greater than the amount accomplished in the second period of the same duration. The action proceeds as it were with a rush at first; it then gradually becomes more and more slow." 2, "that the equation

$$CaCl_2+M_2CO_3=2MCl+CaCO_3$$

does not furnish a full expression of the action of sodium or potassium carbonate upon calcium chloride. When the two salts are mixed in the proportion expressed by their respective formulæ, the action represented in the equation is not completed even after . . . forty-six hours. In order fully to realize the equation the mass of the alkaline carbonate must exceed that which would be actually required to transform the whole of the calcium chloride into carbonate were the equation strictly true. When two molecules of potassium carbonate are present to one molecule of potassium chloride the action is complete after the lapse of an hour; were sodium carbonate employed it would be necessary to use rather more than three molecules . . . in order to complete the action in the same period of time. If the mass of the alkaline carbonate be four times that represented in the equation the action is complete in five minutes." 3, "that increasing the mass of the individual molecules of the precipitant tends to increase the amount of chemical change brought about in a stated period of time," the results indicating that when potassium carbonate is used a larger quantity of calcium carbonate is formed in a given time than when sodium carbonate is employed, except when the action is allowed to proceed during very extended periods. 4, that elevation of temperature tends to increase and on the other hand that dilution of the solutions tends to diminish the rapidity of the change.

These last results are cited in favor of the theory that the action depends upon the motion of the molecules in solution and "that any circumstances which would increase the excursions of the residual molecules of sodium or potassium carbonate and of calcium chloride, would also increase the chances in favor of collision occurring between these molecules, and hence the chances in favor of the production of larger quantities of insoluble calcium carbonate. On the other hand, any circumstances tending to decrease the excursions of these residual molecules, or tending to increase the number of molecules which take no part in the production of calcium carbonate, and hence to decrease the chances of the sodium or potassium carbonate and the calcium chloride molecules coming into collision, would also tend to diminish the amount of calcium carbonate produced. Raising the temperature at which the two salts are allowed to react upon one another would tend to increase the number of excursions of the different molecules; diluting the solutions would tend to decrease the chances of collision between the two sets of molecules." J. P. C., JR.

13. *Nitrification.*—In the Journal of the Chemical Society just referred to, Mr. Robert Warrington gives the results of a very interesting series of experiments which confirm the theory that nitrification is due to the action of an organized ferment and is probably a function of some low form of vegetable life. This theory was regarded as probable by Pasteur as long ago as 1862, and during the last year has been substantiated by Schloessing and Müntz (Compt. rend., lxxxiv, 301). These experimenters had sought to establish two points first, that antiseptic vapors prevent nitrification; secondly, that nitrification may be induced by seeding with a substance already nitrifying. Mr. Warrington fully confirms the results of Schloessing and Müntz on both these points and adds the important fact that darkness is apparently essential to the action of the nitrifying germs. In a postscript to his paper under date of January 4th, Mr. Warrington refers to a paper of Messrs. A. Downes and T. P. Blunt, read before the Royal Society on December 6th, in which it is shown that light is inimical to the development of bacteria, a few hours exposure to daylight being in many cases sufficient to destroy all the germs existing in an organic fluid, while similar solutions kept in darkness developed bacteria freely. The bearing of this observation on the fact that light greatly retards or even prevents nitrification is obvious. The influence of light on nitrification was not apparently quite unknown; it is twice hinted at in Gmelin's Chemistry (Cavendish Translation, iii, 68, vii, 92) but it is not mentioned by most writers on the subject. J. P. C., JR.

14. *Re-examination of some of the Haloid Compounds of Antimony*, by Professor J. P. COOKE, Jr. (Proceedings of the American Academy of Arts and Sciences, vol. xiii. Boston, 1877).—An extended abstract of Professor Cooke's paper upon the atomic weight of antimony has been given in this volume of the Journal, pp. 41, 107. The present paper contains a description of the chemical and crystallographical relations of the iodide of antimony in particular, but also of the chloride, bromide, and the oxichlorides, oxibromides, and oxi-iodides. The chloride and the bromide of

antimony were both obtained by several different methods, in distinct crystals. These were extremely deliquescent, but, notwithstanding, measurements were made of sufficient accuracy to prove that they both belonged to the orthorhombic system, and that in their axial lengths they were closely related. The iodide was obtained in three crystalline conditions—hexagonal, orthorhombic and monoclinic. The *hexagonal* crystals have a deep ruby-red color; their specific gravity is 4·848, and the melting point is 167°. The *orthorhombic* crystals have a greenish-yellow color; they are formed when the iodide is volatilized at a low temperature—below 114°, and when subjected to a higher temperature they change into the red, hexagonal modification.

In discussing the morphological relations of the hexagonal iodide, and the orthorhombic chloride and bromide, Professor Cooke finds that though belonging to a different crystalline system, the iodide "is constructively closely isomorphous with the two allied compounds." The relation shown to exist is analogous to that which has often been shown to connect an orthorhombic prism of 60° or 120° with a hexagonal form.

The author calls attention to the views previously expressed by him where he had showed "that by a kind of inter-laminar macling the orthorhombic crystals of foliated minerals frequently imitate the structure as well as the forms of the hexagonal system." The theory had also been advanced by him that the true hexagonal forms might be the result of a "molecular macling," according to which theory the crystalline molecules of these forms are groups of three simpler molecules, each of which is a unit in itself, and which probably always becomes isolated when the substance is converted into vapor. It is assumed that the members of these groups are united among themselves in the same relative positions as the diagonals of a regular hexagon, so that the optically uniaxial character of hexagonal crystals is an effect of such grouping, and the hexagonal form an obvious result of the juxtaposition of the six-sided group. Further it is supposed that the simpler molecules are of such a nature that when united as individuals in positions parallel to each they would form crystals having a rhombic section of 60° and 120°.

The discovery of the orthorhombic modification of the iodide, having angles of 60° and 120°, and at the same time optically biaxial, and the fact that it changes by a slightly elevated temperature into the hexagonal variety, is considered by the author as strong confirmation of the theory he has advanced. It being thus shown that the same external form is compatible with the differences which distinguish the orthorhombic and hexagonal system, and the conditions under which one of these isomers changes into the other, indicate clearly that the difference between the two substances is simply a difference in the grouping of the same molecules, and also that in the red modification the molecules are more intimately united than in the yellow.

The monoclinic variety of the iodide is obtained in crystals in the following manner:—A solution of the iodide in carbon disulphide, after it has been exposed for some hours to the action of the sunlight, undergoes partial oxidation, some iodine being set free, this solution is then distilled over a water bath, and the process continued until almost the whole of the free iodine is removed; the residue is again dissolved in the same solvent, the solution allowed to evaporate spontaneously, there is then obtained a large crop of the red hexagonal crystals, and finally a small quantity of the yellow monoclinic variety. They have a specific gravity of 4·768. That they are true isomers of the other forms of the iodide is shown by chemical analysis, and more strikingly by the ease with which they change into the red hexagonal variety (like the orthorhombic) at a temperature of about 125°.

The form of the monoclinic crystals is in some respects closely related to that of the orthorhombic crystals, for instance in the dimensions of the basal section. Moreover in the re-crystallization of the monoclinic iodide in pure carbon disulphide the hexagonal iodide is obtained, as also a small quantity of minute rhombic plates, whose angles were 60° and 120°. From these facts it is concluded that the monoclinic variety differs chemically from the orthorhombic only in containing a small amount of impurity. The molecules are supposed to be similarly constituted and similarly grouped, but by the adhesion of the impurity to the molecules a certain difference in form results.

The suggestion is made that possibly the supposed orthorhombic crystals may prove upon more exact determination on better material, to be really monoclinic, so that the difference between the two yellow varieties that have been named would exist only in habit. This would not, however, affect the general result reached as to the relation of their form to the hexagonal kind and the reason for its existence.

The "molecular macling" which has been explained is quite distinct from the "interlaminar macling," described by the author in his paper upon the Vermiculites, and alluded to above. The latter involves no essential change in substance; the former produces a new, though isomeric compound. The red and yellow iodides of antimony are as different substances as calcite and arragonite, and the difference in the two cases is conceived to be of the same kind.

E. S. D.

15. *The Telephone, an Instrument of Precision.*—The applications to which the telephone may in future be put cannot yet be all foreseen. I have to-day had its value shown to me in a remarkable way. 1. I used a thermo-electric intermittent current by drawing a hot end of copper wire along a rasp completing the circuit. A telephone was put into the circuit, in another room, and every time that the wire was drawn along the rasp a hoarse croaking was heard in the telephone. 2. I used a thermopile with a Bunsen burner shining on it from a distance of six feet. The current was rendered intermittent by the file, and the sound was most distinctly heard. A Thomson's reflecting galvanometer was introduced into the circuit which showed that the currents

were extremely small. 3. The feeblest attainable currents were now tried. The thermopile was removed, and without any artificial application of heat it was shown by the galvanometer that the natural differences in the temperatures of the different junctions in the circuit were sufficient to generate feeble electric currents only just perceptible with the mirror galvanometer. These were easily detected by aid of the rasp and the telephone. Even when contact was simply made and broken with the hand, a click was heard in the telephone. 4. Lastly, these feeble currents were rendered still more insignificant by passing them through the body of a friend who held one end of the wire in each hand, and still the effects were faintly audible. Here the galvanometer, which was still in circuit, hardly gave any indication.

I have now added the telephone to the list of apparatus in the laboratory, considering it to be perhaps the most delicate test of an electric current which we possess.

In these experiments only one telephone is used, viz: at the receiving end. Employed in this way with a powerful current sent from the other end of the line, we may hope to have messages sent through submarine cables much more rapidly than at present. Probably it will be best to have the intermittent nature of the current maintained by an induction-coil, or by a spring rubbing against a continuously rotating cog-wheel, when the current is allowed to pass only when required by the depression of a key which communicates to the listener at the receiving end the long and short dashes of the Morse alphabet.

I ought to mention that I believe the person who first used a thermo-electric current with a telephone was Prof. Tait.

George Forbes, Andersonian College, Glasgow, February 13. —*From Nature of Feb.* 28.

II. Geology and Mineralogy.

1. *Origin of the Driftless Region of the Northwest;* by Roland D. Irving.—In a notice of vol. ii of the Geology of Wisconsin, given in this Journal for January last, the explanation I have offered for the existence of the driftless region of Wisconsin and the adjoining States is said to be, "though different," closely related to that given by Professor Winchell in his Minnesota Geological Report for 1876. I am informed, also, that a review of the Wisconsin Report, written for a Chicago journal —but which I have not myself seen –has said that my explanation is the same as his. From these statements it might be inferred that I had merely followed in his track. It is entirely true that his views have some analogy to mine, and that they were first published; but my report had all been, not only written, but stereotyped and *printed* before the Minnesota volume came to hand, in the fall of 1877; so that, whatever my conclusions are worth, they are wholly my own. Possibly they are worth the more because analogous to those reached independently by some

one else. The matter has long excited my attention, the entire inadequacy of the older views having forced itself upon me when first engaged in field-work in Wisconsin.

However, though cheerfully acknowledging the priority of Professor Winchell's views so far as they cover the same ground with mine, I find, on examining his report again more closely that not only do we reach our conclusions in quite different ways, but that the analogy between his theory and mine is, after all, not much more than slight; and, moreover, that in some of the main points of his I cannot acquiesce at all. His idea is, as I understand it, that the large area of Archæan rocks lying south of Lake Superior exerted a protecting influence upon the region to the southward, crowding back the ice, which had been split by the high ridge of Keweenaw Point, one part flowing westward on the north side of the point, and so passing by the driftless region on the north, the other turning southward, and so to the *east* of the driftless area. Now I assign *nothing* to the protecting influence of the crystalline rock region, but *everything* to the deflecting influence of the already existing depressions of Lake Michigan, the Green Bay Valley and Lake Superior. The crystalline rock region is no such lofty one as he appears to suppose, is almost everywhere heavily drift-covered, and over most of its surface shows sign of the strongest glaciation. The deep trough of Lake Superior, over 2,000 feet in depth in its western portions, allowed the passage from it of only the uppermost portions of the ice. These, divided into separate tongues by the transverse ridge topography of the region south of the Lake, and by it deflected toward the W.S.W., overtopped the water-shed, and carried morainic drift far down on the southern slope; but, because of their diminished size and force, and of their westerly direction, they left the region farther south untouched.

Professor Winchell's view was reached by noticing the relation in position of the driftless region and the area of crystalline rocks, which relation, in my view, is an accidental one, except so far as the position of the trough of Lake Superior, carved in the soft sandstones of the Lower Silurian and Keweenian, is affected by the position of the Archæan area. My own conclusions were reached by the absolute demonstration of the former existence of separate glaciers in the troughs of Green Bay and Lake Michigan, which is afforded, as I think, by the facts given by Professor Chamberlin and myself in our reports on Eastern and Central Wisconsin. At the time that I wrote, the proofs of a similar state of affairs for the Lake Superior country were nearly as good, and are now complete, as will be seen when the reports on that country come to be published.

University of Wisconsin, February 18, 1878.

2. *Second Geological Survey of Pennsylvania.*

(1) *Report of Progress in the Fayette and Westmoreland district of the Bituminous Coal Fields of Western Pennsylvania;* by *J.* J. STEVENSON. Part 1, Eastern Allegheny County and Fayette

and Westmoreland Counties, west from Chestnut Ridge. 438 pp. 8vo, with maps and sections.—Professor Stevenson's Report gives first an account of the physical features and general geology of the district, and then describes with detail the stratification of the rocks in its different parts, the relations of the several beds to those of the adjoining regions, the distribution of the included coal and iron-ore beds, and various details as to the special characteristics and variations of the beds. The region lies just to the east of Pittsburgh and hence includes the great Pittsburgh coal bed. The coals and iron ores are described, and many analyses are given of the latter.

(2.) *Report of Progress in the Cambria and Somerset District of the Bituminous Coal Fields of Western Pennsylvania;* by F. and W. G. PLATT. Part II, Somerset. 348 pp. 8vo. Harrisburg, Pa.—The region here described lies to the southeast of that treated of in Prof. Stevenson's report. It contains to a large extent the same rocks and coal beds, but with many peculiarities in their distribution that give special interest to the facts brought out. Numerous sections are given which are carefully described, and the subject of the variability of the coal beds is treated of. The volume contains a preface by Professor J. P. Lesley, Director of the Geological survey of the State, announcing that the "Elk Lick Coal," stated in the final Geological Report of 1858 to occur at the bottom of the Barren Measures, and at the top of the Lower Productive Coal series, has no existence, and that it should be expunged from that report wherever it occurs, and also from Lesley's "Manual of Coal." The Report is illustrated by numerous wood-cuts, and six maps and sections.

(3.) *Oil Well Records and Levels;* by JOHN F. CARLL. 348 pp. 8vo. Harrisburg, Pa.—This Report is a very valuable systematised statement of facts connected with the oil deposits of Western Pennsylvania. It contains the geological and geographical positions and depths of all the oil openings, and sections of the rocks in each case as far as they were obtainable. The work gives records, "more or less doubtful," of 2,000 wells; and Prof. Lesley observes, in a prefatory note, that the number might have been 20,000, if the records had been in all cases preserved. This volume is to be followed by another by Mr. Carll in which all the facts here given will be combined and explained in a manner better adapted to the popular reader; and in it the geology of the rocks penetrated by the oil borings, and of the oil level, will be specially considered, with other matters of scientific and practical value.

3. *The Ancient Life-history of the Earth,* by Professor H. ALLEYNE NICHOLSON, Professor of Natural History in the University of St. Andrews, Scotland. 408 pp. 8vo. (New York: Appleton & Co.).—The work is, as the title page states, a comprehensive outline of the principles and leading facts of Paleontological science. After chapters treating in a general way of the fossiliferous rocks, their succession and the uses and relations of the fossils

they contain, it takes up in succession the life of the periods in geological history, commencing with the oldest. Its many illustrations are not as well engraved as they should be. The author is a zoologist as well as geologist, and the student will find his work a very valuable help toward obtaining a comprehensive knowledge of the progress of life on the globe.

4. *Reports of the United States Exploration of the 40th parallel*, CLARENCE KING, Geologist in charge. Submitted to the Chief of Engineers and published by order of the Secretary of War, under authority of Congress. Two volumes of these Reports, on the region in the vicinity of the 40th parallel between the Sierra Nevada and the Front Range of the Rocky Mountains, have recently been issued. They add greatly to our knowledge of Rocky Mountain geology, and are hastening on the time when we shall have a completed map of the great territories. These two volumes are numbered volumes ii and iv. Volume iii, by Mr. King, is now in the press, and will soon be ready for delivery.

Volume ii, contains the "*Descriptive Geology*" by ARNOLD HAGUE and S. F. EMMONS; it is an octavo volume of 890 pages and is illustrated by twenty-six plates. A notice of it is deferred to another number of this Journal.

Volume iv, which extends to 670 pages, consists of three reports or parts: I, *Paleontology*, by F. B. MEEK, illustrated by seventeen plates; II, *Paleontology*, by JAMES HALL and R. P. WHITFIELD, illustrated by seven plates, and III, *Ornithology* by ROBERT RIDGWAY. The Report of the late Mr. Meek contains fossils ranging from the Lower Silurian to the Tertiary. The oldest species are two Trilobites from Antelope Springs, House Range, Utah, named *Conocoryphe* (*Ptychoparia*) *Kingii*) Meek, and *Paradoxides? Nevadensis* Meek.

Professor Hall and Mr. Whitfield describe Potsdam fossils from the Eureka District, Nevada; and the west side of Pogonip Mountain, White Pine District, Nevada. They include Brachiopods, of the genera *Obolella*, *Lingulepis*, *Kutorgina* and *Leptæna*, also Trilobites of the genera *Crepicephalus* (7 species), *Conocephalites* (*Pterocephalus*) (1 sp.), *Ptychaspis* (1 sp.), *Chariocephalus* (1 sp.), *Dicellocephalus* (3 sp.), *Agnostus* (4 sp.) The other fossils belong to the upper portion of the Lower Silurian, the Devonian, Carboniferous, Triassic and Jurassic formations.

Mr. Ridgway's report contains much on the habits of the various birds observed by him while in the field, and also on their distribution and variations. A general review of the several local regions and their faunas is presented, and lists of the species of each, which we propose to notice in a future number.

5. *Report on the Clay Deposits of Woodbridge, South Amboy, and other places in New Jersey, together with their uses for fire brick, pottery, etc.*, by GEORGE H. COOK, State Geologist of New Jersey. 382 pp. 8vo. Trenton, N. J., 1878.—This valuable volume, on the New Jersey clay deposits, is published by the State as a part of the results of the Geological Survey. It gives,

in the first place, the general geological and geographical facts connected with the clays, and afterwards describes the various openings, and the character and composition of the clays they afford. It closes with a chapter on the economical uses of the clays and on the methods of digging. It has two maps. One shows the distribution of the clays across the State of New Jersey, along a band, five to eight miles wide, from South Amboy, southwest to Trenton, on the Delaware. The second is a large colored map, giving special details with regard to the northeastern portion of this tract, and covering a surface of about ten miles square, with contour lines for every ten feet.

6. *The First and Second Glacial eras of Europe.*—The third number for 1878 of Petermann's "Geographischen Mittheilungen" contains a colored chart, showing the condition of Europe in the first and second Glacial eras, according to the views of H. Habenicht, prepared, the author states, from the best sources. It represents, by colors, the distribution of the glaciers of the two eras, and their difference in length. Mr. Habenicht holds, contrary to the opinions of many geologists, that the boulders were distributed by icebergs; and consequently he places Northern Europe, down to nearly latitude 50°, under the sea, and contracts the glaciers of Scandinavia to the valleys on the western side of the range. The chart is interesting as exhibiting the supposed condition of Europe, according to one believing in the iceberg theory of the drift.

7. *Cordaites with flowers from the Coal region of Pennsylvania.*—Mr. Lesley communicated part of a letter from Mr. L. Lesquereux, of Columbus, Palæobotanist of the Geological Survey of Pennsylvania, relating the recent discovery by Mr. Mansfield, in his coal mines near Darlington, Beaver Co., Pa., of the flowers and leaves attached to the stems of *Cordaites.* "Sternberg in 1835, first found a stem with leaves attached; on which specimen, Corda, in 1845, made his celebrated analysis of *Flabellaria borassifolia.* One other specimen was found by Lesquereux near Potsville in 1858. Recently, Grand 'Eury's discoveries enabled him to publish last fall his splendid monograph of the *Cordaites* in his Carboniferous Flora. Mr. Mansfield has now obtained a splendid series of branches with leaves, and even with leaves and flowers, representing in well defined characters several species, and a new section of this family unknown to Grand 'Eury; so that we now have not only the types of the French author, but other and some new ones far more clearly illustrating the relation of this remarkable group."—*Amer. Phil. Soc.*, Feb. 1, 1878.

8. *On the Mineral Caves of Huallanca, Peru;* by HENRY SEWELL, F.R.G.S.—Mr. Sewell has given in the London Mining Journal an account of his visit, made in 1875, to the mineral caves of Huallanca. The silver-producing caves are situated upon the eastern flank of the Peruvian Andes at an altitude of 14,700 feet above the sea, and 4000 feet above the town of Huallanca. The region was reached after an arduous journey across the Andes

from the port of Casma; in the course of it, it was necessary to cross several parallel ranges, one of them 16,800 feet in altitude, and another 17,200 feet. The silver mines lie in the heart of a coal formation which has been upheaved so that the strata now stand nearly vertical. Numerous seams of coal occur near the mines. The mass of the argentiferous ores consists of the mineral tetrahedrite, and they contain about 800 ozs. to the ton. The ore is obtained in the shaly portion of the formation, as well as in the sandstone. In the latter it is found in huge "vugbs" or caves, many of these being as much as twenty-five to thirty feet long, and of about the same depth. These caves are coated with from two to three inches of argentiferous ores and millions of crystals of tetrahedrite are destroyed by the picks of the miners who break down the ore in that manner. [The specimens of tetrahedrite brought back by Mr. Sewell are very remarkable for the unusual size and brilliancy of the crystals; some of these he has presented to the Yale Museum].

9. *On the new Mineral Homilite.*—In December, 1876, M. Paijkull published an account of a new mineral, associated with meliphanite and erdmannite on the island of Stockoë, near Brevig, Norway, to which he gave the name of *homilite.* The mineral was of a black or brownish-black color, and in composition was shown to be related to datolite, it containing a considerable amount of boracic acid. The same mineral has been investigated by MM. Des Cloizeaux and Damour (Ann. Chem. et Phys., V, xii, 1877). M. Des Cloizeaux has shown that in crystalline form it is closely isomorphous with gadolinite and datolite. Moreover the optical examination has shown it to be still more nearly related to the former mineral in that some crystals are entirely doubly refracting, others have a yellow crust of singly-refracting mineral, and others are totally isotropic. M. Damour has analyzed the mineral and his analysis (2) is here quoted together with that of M. Paijkull (1).

	SiO_2	B_2O_3	Al_2O_3	Fe_2O_3	FeO	MnO	Ce_2O_3*	MgO	CaO	Na_2O	K_2O	H_2O	
(1)	31·87	[18·08]	1·50	2·15	16·25	..	..	0·52	27·28	1·09	0·41	0·85	=100·
(2)	33·00	[15·21]	..	..	18·18	0·74	2·56	..	27·00	1·01	..	2·30	=100·

* With La_2O_3, Di_2O_3.

The hardness is 4·5, and the specific gravity 3·34 (Damour). From the fact that the analyses do not afford any simply atomic relations, and from the results of his optical examination, M. Des Cloizeaux concludes that the singly refracting portions of the crystals have been so changed as to form with doubly refracting parts a mixture of varying proportions and hence of variable composition.

III. BOTANY AND ZOOLOGY.

1. *Flora of Tropical Africa;* by DANIEL OLIVER. Vol. III. *Umbelliferæ* to *Ebenaceæ.*—In the present continuation of this work Professor Oliver has secured an efficient collaborator in Mr. Hiern, who has not only taken the *Ebenaceæ,* of which he has formerly published a classical monograph, but also the *Umbel-*

liferæ, *Rubiaceæ* and *Dipsaceæ*, and has borne a part in the elaboration of the *Compositæ*. The latter order is represented by more genera than would be expected, namely 117, and 17 are peculiar to this Flora, all small or monotypic. The only large genus is *Vernonia*, with 78 species. The *Rubiaceæ*, which occupy a slightly larger number of pages (215) have 78 genera, 30 of which are endemic, and three are new. We observe that *Tarenna* Gærtn., replaces *Webera* of Schreber, being three years older; also *Chomelia* Linn., is said to be the correct name, but *Chomelia* of Jacquin is kept up. *Canthium* Lam., is restored in good time for that important genus, much to the relief of the nomenclature, and the original *Plectronia* of Linnæus is said to be *Olinia!* The Liberian coffee, the seed of *Coffæa Liberica*, "is said to be far superior to *C. Arabica*, having larger berries and a finer flavor, and being at the same time more robust and productive." *Psychotria*, with 61 species, is made to include *Chasalia*, and to exclude *Grumilea*, which is restricted to Old World species. *Richardia* Linn., is taken up in place of *Richardsonia* Kunth, to the détriment of the common *Aroideous* genus which has so long borne this name. It is well known and duly recorded that the Rubiaceous genus was named in honor of Mr. Richardson; wherefore it should have been written *Richardsonia*. There was not absolute need to change the form; but this having long ago been done, and another plant named *Richardia*, in honor of L. C. Richard, and the present state of things having been acquiesced in for more than half a century, this certainly is a case to which the rule "*quieta non movere*," and Lord Melbourne's deprecation, "Can't you let it alone?" would well apply. A. G.

2. *Ferns of North America;* by Prof. D. C. EATON. Part III has appeared with remarkable promptitude, and has more than usual interest. There is a characteristic plate of *Aspidium Noveboracense;* another devoted to *Camptosorus rhizophyllus*, and to *Aspidium pinnatifidum* (to the latter of which, rather than to the former, *A. ebenoides* may have some blood-relationship); and the third is given to *Notholæna Fendleri* and *N. dealbata*, both species well adapted for color-printing by their whitened fronds, delicate subdivision, and brown stalks. A. G.

3. *Report on the Fossil Plants of the Auriferous Gravel Deposits of the Sierra Nevada;* by LEO LESQUEREUX. With 10 plates. 4to. 1878. (Vol. vi, No. 2 of the Memoirs of the Museum of Comparative Zoology at Harvard College.)—A memoir beautiful in its dress and very interesting in character. The specimens figured are impressions of leaves, "imbedded in a fine-grained whitish clay or soapstone," mainly from a collection made by Mr. Voy, of Oakland, California, which was secured and presented to the California University by the liberality of Mr. D. O. Mills. These Pliocene remains are of special interest from the light they throw upon the archeology of the existing North American flora; and we shall probably find occasion and opportunity to discuss them in this relation. A. G.

4. Elias Magnus Fries of Upsal, and one of the most distinguished and revered of botanists, born on the 15th of August, 1794, died February 8, 1878. He had worthily occupied the chair of Linnæus at the University of Upsal for almost half a century; and his death, a month after the celebration of the 100th anniversary of that of Linnæus, marks the close of an era. A. G.

5. *Catalogue of the Collections in the Museum of the Pharmaceutical Society of Great Britain*, compiled by E. M. Holmes, F.L.S., Curator of the Museum. 1878. pp. 302.—About two-thirds of this catalogue relate to the vegetable materia medica; the objects all classed under the names of the plants producing them with interesting descriptive and critical remarks. A. G.

6. *Thuret's Garden at Antibes*, as already announced, has been turned over to the French Government, in order that its usefulness may be continued. It is now decided that it is to be a sort of Mediterranean branch of the Jardin des Plantes of Paris. Botanists will be glad to know that M. Naudin, the best of horticultural botanists, is to have the charge of it. The climate of Antibes is especially proper for Utah, Arizonian and South-Californian plants; and we trust our collectors and amateurs will keep this in mind, and that many seeds, bulbs, succulent plants, etc., may be supplied to this important establishment, where they may be kept in cultivation. The Botanic Garden of Harvard University will receive and forward any American collections destined for it. A. G.

7. *Circumspection of Ants.*—Professor Leidy states that, a number of years ago, he had been led to an observation on the little red ant, sometimes a great pest to our dwellings, which would indicate a ready disposition to become circumspect. When he purchased his present residence, while it was undergoing some repairs, he noticed a fragment of bread, left by a workman in one of the second-story rooms, swarming with ants. Apprehending that the house was seriously infested, to ascertain whether it was so he placed a piece of sweet cake in every room from the cellar to the attic. At noon every piece was found covered with ants. Having provided a cup of turpentine oil, each piece was picked up with forceps and the ants tapped into the oil. The cake was replaced, and in the evening was again found covered with ants. The same process was gone through the following two days, morning, noon, and night. The third day the number of ants had greatly diminished, and on the fourth there were none. He at first supposed the ants had all been destroyed, but in the attic he observed a few feasting on some dead house flies, which led him to suspect that the remaining ants had become suspicious of the sweet cake. He accordingly distributed through the house pieces of bacon, which were afterwards found swarming with ants. This was repeated with the same result for several days, when, in like manner with the cake, the ants finally ceased to visit the bacon. Pieces of cheese were next tried, with the same results; but with an undoubted thinning in the multitude of ants. When the

cheese proved no longer attractive, recollecting the feast on dead flies in the attic, dead grasshoppers were supplied from the garden. These, again, proved too much for the ants; and, after a few days' trial, neither grasshoppers nor anything else attracted them; they appear to have been thoroughly exterminated, nor has the house since been infested with them.—*Proc. Acad. Nat. Sci. Philadelphia*, 1877, p. 320.

8. *Carboniferous Articulates of Illinois.*—At the meeting of the Boston Society of Natural History for December 5, 1877, Mr. S. H. Scudder showed some drawings of interesting Articulates from the Carboniferous rocks of Illinois. The first represented a species of white ant, showing a wing without reticulation; the second, the terminal segments of a crustacean belonging to a genus allied to *Dithyrocaris*, but which he had at first taken for some extraordinary form of insect.

9. *A Manual of the Anatomy of Invertebrated Animals*, by THOMAS H. HUXLEY, F.R.S. 596 pp. 8vo. (New York: Appleton & Co.)—The best and latest students' manual of the invertebrates in the English language.

10. *Nomenclature in Zoology and Botany*, by W. H. DALL, U. S. Coast Survey.—Mr. Dall, as chairman of the committee of the American Association on Zoological Nomenclature, has been doing good service to natural science by his efforts to obtain the opinions of naturalists on various points connected with the subject of nomenclature. This pamphlet is the result of further labor in the same direction. It contains, besides a brief report, an Appendix consisting of a resumé of all the principles and rules of nomenclature as heretofore set forth by the chief authorities on the subject, with initials giving the authority for the diverse views, in preparing which the author has evidently expended a great amount of labor and care. He has endeavored, moreover, to combine the principles of zoological and botanical nomenclature into one symmetrical system. Mr. Dall's statement of this subject is a great step toward realizing the uniformity so much desired.

11. *A Pigeon living after the removal of nearly all the Brain.*—Dr. McQuillen described a case of the extirpation of nearly all of the cerebrum of a pigeon by himself, and desired to place on record the fact that the subject not only survived the operation twenty-four days, but gradually regained its usual powers and habits of flight, and its ability to feed itself and drink. Only one such case is on record. He argued for the propriety and usefulness of such operations from the acknowledged existing uncertainties of the science.—*Proc. Amer. Phil. Soc.*, Feb. 1, 1878.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Earthquake of November* 4, 1877.—(Read at the November meeting of the Natural History Society, by PRINCIPAL DAWSON, LL.D., F.R.S.)—In the Canadian Naturalist, vol. v, first series will be found notes on the earthquake of October 17,

1860, with a summary of facts relating to the previous shocks recorded in Canada, and some general remarks on their periods, local peculiarities and probable causes. The subject was continued in vol. i, of the new series, in connection with the earthquake of April, 1864, and in vol. v, new series, in connection with that of October 20, 1870. I may refer to these notices for what is known on Canadian earthquakes up to that time, and we may now continue the narrative in connection with the somewhat wide-spread disturbances of the earth's crust in the present autumn.

On January 4, 1871, a shock was experienced at Hawkesbury, Ontario, but was not reported from any other place. A more extensive earthquake occurred on May 22, 1871. It prevailed from the city of Quebec to the western part of Ontario. The time for Quebec is stated at ten minutes before two A. M., and there was a second shock at twenty minutes past three. The time for Perth, Ontario, is stated at half-past one. It is noteworthy that this earthquake occurred at nearly the same time with that recently experienced. Since 1871 several minor shocks have been noticed from time to time, but did not attract much attention, and I have preserved no details in relation to them.

That of the present month was probably the most considerable since 1871. It occurred at Montreal, at ten minutes before two on the morning of Sunday, November 4. At Montreal there was only one distinct shock, preceded by the usual rumbling noise, and sufficiently severe to be distinctly felt, and to shake window-sashes and other loose objects, causing them to vibrate for several seconds. In so far as the published reports give information, the shock would seem to have been limited to the area along the river St. Lawrence, extending from near Three Rivers on the east, to Kingston on the west, and in a direction transverse to the St. Lawrence from Ottawa to the southern part of New England. In a paper prepared for the American Journal of Science, by Professor Rockwood, of Princeton, he defines the area in question as that of "an irregular trapezium whose angles are marked by Pembroke, Ont., Three Rivers, P. Q., Hartford, Conn., and Auburn, N. Y., and which is some 200 miles on its northern and southern side, about 300 miles on the eastern side, and 175 on the western." So far as can be learned from the reports, the shock seems to have been most severely felt on the north side of the valley of the St. Lawrence and about Lake Champlain, or may be said to have had its center in the Adirondack and Green Mountain region.

In the notice of Canadian earthquakes in 1860, I mentioned that it had been observed that the greatest and most frequent shocks have occurred a little after the middle and toward the close of each century. We are now approaching the latter period, so that possibly the last shock may be the beginning of a series of similar phenomena. Since, however, there is no known reason for this periodicity, it may be a merely accidental coincidence, or may depend on some cycle of about half a century.

If we add to the table of earthquakes in Eastern America, given in vol. v of the Naturalist, the more recent earthquakes observed in Canada, the proportion for the several months will stand as follows:—

January, 9 earthquakes; February, 4; March, 5; April, 5; May, 7; June, 3; July, 4; August, 6; September, 4; October, 8; November, 15; December, 8. Total, 78.

Thus of seventy-eight recorded Canadian and New England earthquakes, fifteen, or nearly one-fifth, occurred in November; forty, or more than half of the total number, in the third of the year, extending from October to January inclusive. The published catalogues show that similar ratios have been observed elsewhere, at least in the Northern hemisphere.

In some earthquakes a low state of the barometer has been observed, as if a diminution of atmospheric pressure was connected with the movements of the crust producing seismic vibrations. This we can readily understand if a low state of the barometer should prevail over an area of the crust tending to rise, simultaneously with a high pressure over a sinking area. In this case a state of previous tension might terminate in a rent of the crust causing vibration. In the present case no very decided indication of such a cause appears, at least in so far as this part of the St. Lawrence Valley is concerned. Mr. McLeod informs me that the mean barometer for the week preceding the earthquake was 29·7564, and for the following week 30·0864. The barometer on the Friday before the earthquake at 8 P. M. was 29·115, the lowest observed since March last; but at 1·50 A. M. on Saturday it was about 29·967, which is very near the mean of November, 1876, and also a little above the mean barometer of the place for the whole year; and on Sunday afternoon it rose to 30·200. It would thus appear that the earthquake was preceded by a low state of the barometer, and followed by one unusually high for the season, and this rapid fluctuation was accompanied with much atmospheric disturbance in the region of the lakes and the St. Lawrence Valley. The weather map issued by the War Department at Washington for Sunday morning, November 4, shows a low barometer in the Gulf of St. Lawrence and a high barometer in the Middle States—the area of the earthquake being about half way between the extremes.

In connection with previous earthquakes it has been observed that the greatest intensity of the shocks appeared near the junction of the Laurentian with the Silurian formations. This would be a natural consequence either of the propagation of vibrations upward from deep underlying regions through the Laurentian rocks, or from the overlying sedimentary rocks towards these older rocks. In the case of the recent earthquake, this appears to have applied chiefly to the border of the Laurentians extending round by the Ottawa and Kingston to the Adirondacks, as if a wave propagated through the Silurian formations had broken against the southern and eastern sides of the Laurentian region, or a shock originating under the Laurentian of these regions had extended itself from them into the Silurian rocks to the south and east. If the prevailing impression stated in the reports, that the vibrations passed from W. to E. or N.W. to S.E., is correct, the latter would be the more probable supposition. It is, however, very difficult to attain to any certainty as to the actual direction of the disturbance, and some observers give it as precisely the opposite of that above stated.

On the 14th of November a slight shock was felt at Cornwall, Ontario, and on the 15th of November earthquake shocks occurred over a wide area in Kansas, Iowa, Dakota and Nebraska.

OBITUARY.

CHARLES FREDERICK HARTT.—Professor Hartt, according to a telegram from Rio Janeiro, died of yellow fever soon after the middle of March (18th ?). Professor Hartt was born about 1838 at St. John, N. B. In his youth he evinced a taste for geology, and discovered at St. John many fossil plants, and the oldest specimens of fossilized insects then known. He studied from 1862 to 1865 under Agassiz at Cambridge, and accompanied that eminent scientist to Brazil as geologist of his expedition. On his return he was appointed Professor of Geology and Physical Geography at Cornell University. After his first visit to Brazil, Professor Hartt acquired a thorough knowledge of the Portuguese language, and that empire became his favorite field of study. He returned three times and zealously explored the northern provinces, giving most attention to the valley of the Amazon. During one of these expeditions, undertaken under the auspices of the Hon. Edwin D. Morgan of New York, he sent home an interesting series of letters for publication. Papers containing some of the geological results of these expeditions are noticed in this Journal in volumes i, iv, vii, viii and x, of the third series (1871 to 1875). In May of 1875 the Brazilian Government placed Professor Hartt in charge of the Geological Survey of the Empire, and gave him a liberal salary; since then he has been carrying forward this great work. The results thus far obtained have been only partially published. A translation of his first Report of Progress, made by Professor T. B. Comstock, is contained in this Journal in vol. xi, 1876 (p. 466), and a brief announcement of further discoveries, in vol. xii (p. 464). In 1870, he published his principal work, "The Geology and Physical Geography of Brazil." He felt at home in Rio, where he enjoyed excellent health, and was greatly esteemed. His family reside at Buffalo, N. Y. His death is a great loss to the scientific world.

ANGELO SECCHI, the Astronomer and Director of the Observatory at the Collegio Romano at Rome, Italy, died on the 26th of February last. Father Secchi, in the years 1848 and 1849, was connected with the Observatory of Georgetown College, near Washington. In 1850 he returned to Europe and entered on his labors at Rome. His papers on astronomical and physical subjects are very numerous and of great value.

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ART. XLVII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND, Professor of Physics in the Johns Hopkins University, Baltimore, Md.

[Continued from page 291.]

Theory of the Method.

WHEN a current is induced in a circuit by magnetic action of any kind, Faraday has shown that the induced current is proportional to the number of lines of force cut by the circuit and inversely as the resistance of the circuit. If we have two circuits near each other, the first of which carries a current, and the second is then removed to an infinite distance, there will be a current in it proportional to the number of lines of force cut. Let now a unit current be sent through the second circuit and one of strength E through the first; then, on removing the second circuit, work will be performed which we easily see is also proportional to the number of lines of force cut. Hence, if EM is the work done, Q is the induced current, and R is the resistance of the second circuit,

$$Q=CE\frac{M}{R},$$

where C is a constant whose value is unity on the absolute system.

When the current in the first circuit is broken, the lines of force contract on themselves, and the induced current is the same as if the second circuit had been removed to an infinite distance. If the current is reversed the induced current is twice as great; hence in this case

$$Q=2E\frac{M}{R} \text{ or } R=2M\frac{E}{Q}.$$

Hence, to measure the absolute resistance of a circuit on this method, we must calculate M and measure the ratio of Q to E. M is known as the mutual potential of the two circuits with unit currents, and mathematical methods are known for its calculation.

The simplest and best form in which the wire can be wound for the calculation of M is in parallel circular coils of equal size and of as small sectional area as possible. For measuring E a tangent galvanometer is needed, and we shall then have

$$E=\frac{H}{G}\tan\theta,$$

where H is the horizontal intensity of the earth's magnetism at the place of the tangent galvanometer, and G the constant of the galvanometer.

For measuring Q we must use the ballistic method, and we have

$$Q=\frac{H'}{G'}\frac{T}{\sqrt{\pi^2+\lambda^2}}E^{\frac{\lambda}{\pi}\tan^{-1}\frac{\pi}{\lambda}}2\sin\tfrac{1}{2}\theta',$$

which for very small values of λ becomes

$$Q=\frac{H'}{G'}\frac{T}{\pi}2\sin\tfrac{1}{2}\theta'(1+\tfrac{1}{2}\lambda-\tfrac{1}{6}\lambda^2)$$

$$\therefore R=M\frac{H}{H'}\frac{G'}{G}\frac{\pi\tan\theta}{T\sin\frac{1}{2}\theta'}\frac{1}{1+\frac{1}{2}\lambda-\frac{1}{6}\lambda^2},$$

where H′ is the horizontal component of the earth's magnetism at the place of the small galvanometer, G′ its constant, T the time of vibration of the needle, and λ the logarithmic decrement.

The ratio of H′ to H can be determined by allowing a needle to vibrate in the two positions. But this introduces error, and by the following method we can eliminate both this and the distance of the mirror from the scale by which we find θ′ and the error of tangent galvanometer due to length of needle. The method merely consists in placing a circle around the small galvanometer and then taking simultaneous readings with the current passing through it and the tangent galvanometer, before and after each experiment. Let α and α′ be the deflections of the tangent galvanometer and the other galvanometer respectively, and let G″ be the constant of the circle at the point where the needle hangs, then

$$\frac{H}{G}\tan\alpha=\frac{H'}{G''}\tan\alpha',$$

and we have finally

$$R=M\frac{\pi}{T}\frac{G}{G''}\frac{\tan\alpha'}{\tan\alpha}\frac{\tan\theta}{\sin\frac{1}{2}\theta'}\frac{1}{1+\frac{1}{2}\lambda-\frac{1}{6}\lambda^2},$$

which does not contain H or H′, and the distance of the mirror from the scale does not enter except as a correction in the ratio of sin $\frac{1}{2}\theta'$ and tan α'; and, as α and θ can be made nearly equal, the correction of the tangent galvanometer for the length of needle is almost eliminated. When the method of recoil is used, we must substitute $\frac{1}{1+\frac{1}{2}\left(\frac{\lambda}{\pi}\right)^2}$ for the term involving λ, and sin $\frac{1}{2}A'$+sin $\frac{1}{2}B'$ in the place of sin $\frac{1}{2}\theta'$, A′ and B′ being the greater and smaller arcs in that method. This is on the supposition that λ is small.

The ratio of G″ to G must be so large, say 12,000, that it is difficult to determine it by direct experiment, but it is found readily by measurement or indirect comparison.

It is seen that in this equation the quantities only enter as the first powers, and that the only constants to be determined which enter the equation are M, G and G″, which all vary in simple proportion to the linear measurement. It is to be noted also that the only quantities which require to be reduced to standard measure are M and T, and that the others may all be made on any arbitrary scale. No correction is needed for temperature except to M. Indeed, I believe that this method exceeds all others in simplicity and probable accuracy and its freedom from constant errors, seeing that every quantity was varied except G″ and G, whose ratio was determined within probably one in three thousand by two methods.

Having obtained the resistance of the circuit by this method, we have next to measure it in ohms. For this purpose the resistance of the circuit was always adjusted until it was equal to a certain German silver standard, which was afterward carefully compared with the ohm. This standard was about thirty-five ohms.

By this method, the following data are needed.

1. Ratio of constants of galvanometer and circle.
2. Ratio of the tangents of the two deflections of tangent galvanometer.
3. Ratio of the deflection to the swing of the other galvanometer.
4. Mutual potential of induction coils on each other.
5. Time of vibration of the needle.
6. Resistance of standard in ohms.

For correction we need the following:

1. The logarithmic decrement.
2. Distance of mirror from scale.
3. Coefficient of torsion of suspending fiber.
4. Rate of chronometer.
5. Correction to reduce to standard meter.

6. Variation of the resistance of German silver with the temperature.
7. Temperature of standard resistance.
8. Arc of swing when the time of vibration is determined.
9. Length of needle in tangent and other galvanometer (nearly compensated by the method).
10. The variation of resistance of circuit during the experiment.

The following errors are compensated by the method of experiment.

1. The local and daily variation of the earth's magnetism.
2. The variation of the magnetism of the needle.
3. The magnetic and inductive action of the parts of the apparatus on each other.
4. The correction for length of needle in the tangent galvanometer (nearly).
5. The axial displacement of the wires in the coils for induction.
6. The error due to not having the coils of the galvanometer and the circle parallel to the needle.
7. Scale error (partly).
8. The zero error of galvanometers.

Calculation of Constants.

Circle.—For obtaining the ratio of G to G″, it is best to calculate them separately and then take their ratio, though it might be found by Maxwell's method ("Electricity," Article 753). But as the ratio is great, the heating of the resistances would produce error in this latter method.

For the simple circle,

$$G' = 2\pi \frac{A^2}{(A^2+B^2)^{\frac{3}{2}}} = \frac{2\pi}{A}\left(1 - \frac{3}{2}\left(\frac{B}{A}\right)^2 + \&c.\right)$$

where A is its radius and B the distance of the plane of the circle to the needle on its axis.

Galvanometer for Induction Current.—For the more sensitive galvanometer, we must first assume some form which will produce a nearly uniform field in its interior, without impairing its sensitiveness. If we make the galvanometer of two circular coils of rectangular section whose depth is to its width as 108 to 100, and whose centers of sections are at a radius apart from each other, we shall have Maxwell's modification of Helmholtz's arrangement. The constant can then be found by calculation or comparison with another coil.

Maxwell's formulæ are only adapted to coils of small section. Hence we must investigate a new formula.*

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* A formula involving the first two terms of my series, but applying only to the special case of a needle in the center of a single circle of rectangular section, is given by Weber in his "Elektrodynamische Maasbestimmungen inbesondere Widerstandsmessungen," S. 372.

Let N be the total number of windings in the galvanometer.
Let R and r be the outer and inner radii of the coils.
Let X and x be the distances of the planes of the edges of the coils from the center.
Let α be the angle subtended by the radius of any winding at the center.
Let b be the length of the radius vector drawn from the center to the point where we measure the force.
Let θ be the angle between this line and the axis.
Let c be the distance from the center to any winding.
Let w be the potential of the coil at the given point.

Then (Maxwell's "Electricity," Art. 695), for one winding,

$$w=-2\pi\left\{1-\cos\alpha+\sin^2\alpha\left(\frac{b}{c}Q_1'(\alpha)Q_1(\theta)+\frac{1}{2}\left(\frac{b}{c}\right)^2Q_2'(\alpha)Q_2(\theta)+\&c.\right)\right\}$$

and for two coils symmetrically placed on each side of the origin,

$$w=4\pi\left\{\cos\alpha-\sin^2\alpha\left(\frac{1}{2}\left(\frac{b}{c}\right)^2Q_2'(\alpha)Q_2(\theta)+\frac{1}{4}\left(\frac{b}{c}\right)^4Q_4'(\alpha)Q_4(\theta)+\&c.\right)\right\}$$

where $Q_2(\theta)$, $Q_4(\theta)$, &c., denote zonal spherical harmonics, and $Q_2'(\alpha)$, $Q_4'(\alpha)$, &c., denote the differential coefficients of spherical harmonics with respect to $\cos\alpha$.

As the needle never makes a large angle with the plane of the coils, it will be sufficient to compute only the axial component of the force, which we shall call F. Let us make the first computation without substitution of the limits of integration, and then afterward substitute these:

$$F=\tfrac{1}{2}\frac{N}{(R-r)(X-x)}\iint\frac{dw}{dx}dxdr$$

$$F=\tfrac{1}{2}\frac{N}{(R-r)(X-x)}\int wdr,$$

and we can write

$$F=\frac{2\pi N}{(R-r)(X-x)}\left\{H_0+H_2b^2Q_2(\theta)+H_4b^4Q_4+\ \&c.\right\}$$

where $H_0=x\log_\epsilon(r+\sqrt{x^2+r^2})$

$$H_i=-\frac{1.3.5\ ..\ (2i-1)}{i(1.2.3\ ..\ i)}\frac{\sin^3\alpha}{x^{i-1}}\left\{A_{,}\frac{\cos^{2i-4}\alpha}{2i-1}+B_{,}\frac{\cos^{2i-6}}{2i-3}+\ \&c.\right\}$$

$$A_{,}=i$$

$$B_{,}=A_{,}\frac{2i-4}{2i-1}-\frac{i(i-1)(i-2)}{(2i-1)2}$$

$$C_{,}=B_{,}\frac{2i-6}{2i-3}+\frac{i(i-1)(i-2)\ ..\ (i-4)}{(2i-1)(2i-3)2.4}$$

$$D_{,}=C_{,}\frac{2i-8}{2i-5}-\frac{i(i-i)\ ..\ (i-6)}{(2i-i)(2i-3)(2i-5)2.4.6}$$

$$E_{,}=\ \&c.,\ \&c.$$

Substituting the limits for x, r and α, we find

$$H_0 = X \log_e \frac{R+\sqrt{X^2+R^2}}{r+\sqrt{X^2+r^2}} - x \log_e \frac{R+\sqrt{x^2+R^2}}{r+\sqrt{x^2+r^2}}$$

$$H_2 = -\tfrac{1}{2} \left\{ \frac{1}{X}\left(\frac{R^3}{(R^2+X^2)^{\frac{3}{2}}} - \frac{r^3}{(r^2+X^2)^{\frac{3}{2}}} \right) - \frac{1}{x}\left(\frac{R^3}{R^2+x^2)^{\frac{3}{2}}} - \frac{r^3}{(r^2+x^2)^{\frac{3}{2}}} \right) \right\}$$

$$H_4 = -\frac{1}{24} \left\{ \frac{R^3}{X^3(X^2+R^2)^{\frac{3}{2}}}(20X^4+7X^2R^2+2R^4) - \right.$$

$$\frac{r^3}{X^3(X^2+r^2)^{\frac{3}{2}}}(20X^4+7X^2r^2+2r^4) - \frac{R^3}{x^3(x^2+R^2)^{\frac{3}{2}}}(20x^4+7x^2R^2+2R^4)$$

$$\left. + \frac{r^3}{x^3(x^2+r^2)^{\frac{3}{2}}}(20x^4+7x^2r^2+2r^4) \right\}$$

The needle consisted of two parallel lamina of steel of length, l, and a distance, W, from each other. As the correction for length is small, we may assume that the magnetism of each lamina is concentrated in two points at a distance $n\,l$ from each other, where n is a quantity to be determined.

Hence

$$G = \frac{2\pi N}{(R-r)(X-x)} \left\{ H_0 + H_2 \frac{l^2 n^2}{4} Q_2(\theta') + H_4 \frac{l^4 n^4}{16} Q_4(\theta') + \text{etc.} \right\}$$

where $\cos\theta' = \frac{W}{\sqrt{(\frac{1}{2}nl)^2+W^2}}$, seeing that the needle hangs parallel to the coils. In short thick magnets, the polar distance is about $\frac{2}{3}\,l$ and the value of n will be about $\frac{3}{4}$. For all other magnets it will be between this and unity. In the present case $n=\frac{3}{4}$ nearly.

As all the terms after the first are very minute, this approximation is sufficient, and will at least give us an idea of the amount of this source of error.

Induction Coils.

The induction coils were in the shape of two parallel coils of nearly equal size and of nearly square section.

Let A and a be the mean radii of the coils. Let b be the mean distance apart of the coils.

Let

$$c = \frac{2\sqrt{Aa}}{\sqrt{(A+a)^2+b^2}}.$$

Supposing the coils concentrated at their center of section we know that

$$M_0 = 4\pi\sqrt{Aa} \left\{ \left(\frac{2}{c} - c\right) F(c) - \frac{2}{c} E(c) \right\}$$

where F(c) and E(c) are elliptic integrals.

If ζ and η are the depth and width of each coil, the total value of M will be, when $A=a$ nearly,

$$M=M_0+\frac{1}{12}\left\{\frac{d^2M_0}{dA^2}\eta^2+\frac{d^2M_0}{db^2}\zeta^2\right\}+\text{etc.}$$

and we find

$$\frac{d^2M_0}{dA^2}=-\frac{\pi}{A}\left\{\frac{E(c)}{2A(1-c^2)}\left(2Ac+\frac{4b^2c^2}{8A(1-c^2)}(1-3c^2+2c^4)\right)-F(c)\left(c+\frac{b^2c^3}{4A^2}\right)\right\}$$

$$\frac{d^2M_0}{db^2}=\frac{\pi c}{A(1-c^2)}\left\{F(c)\left(2(1-c^2)-\frac{b^2c^2}{4A^2}(2-c^2)\right)-E(c)\left(2-c^2-b^2c^2\frac{1-c^2+c^4}{2A^2(1-c^2)}\right)\right\}$$

Corrections.

Calling β and δ the scale deflections corresponding to $\tan\alpha'$ and $\sin\frac{1}{2}\theta'$, we may write our equation for the value of the resistance

$$R'=\frac{K}{T}\frac{\tan\theta}{\tan\alpha}\frac{\beta}{\delta}\,\frac{1-\frac{1}{4}\left(\frac{\beta}{D}\right)^2+\frac{1}{8}\left(\frac{\beta}{D}\right)^4}{1-\cdot35\left(\frac{\delta}{D}\right)^2+\cdot22\left(\frac{\delta}{D}\right)^4}(1+A+\text{ etc.})$$

where R′ is the resistance of the circuit at a given temperature 17·0° C., and $K=2\pi M\frac{G}{G''}(1+a+b+\text{etc.})$, in which A, B, etc. and *a*, *b*, etc. are the variable and constant corrections respectively.

a. Correction for damping,

$$a=-\tfrac{1}{2}\lambda+\tfrac{5}{12}\lambda^2.$$

b. Torsion of fiber.

The needle of the tangent galvanometer was sustained on a point and so required no correction. The correction for the torsion in the other galvanometer is the same for β and δ and hence only affects T. Therefore, if t is the coefficient of torsion,

$$b=-\tfrac{1}{2}t.$$

c. Rate of chronometer.

Let p be the number of seconds gained in a day above the normal time

$$c=-\frac{p}{86400}.$$

d. Reduction to normal meter. The portion of this reduction which depends on temperature must be treated under the variable corrections. Let m be the excess of the meter used above the normal meter, expressed in meters; then

$$d=+m.$$

e. Correction of T for the arc of vibration. This arc was always the same, starting at c_1 and being reduced by damping to about c_2,

$$e=+\frac{1}{128n\lambda}(c_1^2-c_2^2),$$

where c_1 and c_2 are the total arcs of oscillation.

f. Correction for length of needles. For the tangent galvanometer, the correction is variable. For the circle it is

$$f=+\tfrac{3}{4}\left(\frac{l}{\text{A}}\right)^2$$

where l is half the distance between the poles of the needle and A the radius of circle. For the other galvanometer it is included in the formula for G.

A. Reduction to normal meter. As the dimension of R is a velocity and the induction coils were wound on brass, the correction is

$$\text{A}=+\gamma(t'-t'')$$

where γ is the coefficient of expansion of brass or copper, t' the actual and t'' the normal temperature.

B. Correction of standard resistance for temperature. Let μ be the variation of the resistance for 1° C., t'' be the actual and t^{iv} the normal temperature 17·°0 C.; then

$$\text{B}=-\mu(t'''-t^{iv}).$$

C. Correction for length of needle in tangent galvanometer,

$$\text{C}=+\frac{15}{4}\sin(\alpha+\alpha')\left(\frac{l'}{\text{A}'}\right)^2(\alpha'-\alpha)$$

where l' is half the distance between the poles of the needle and A′ is the radius of the coil.

D. The resistance of the circuit was constantly adjusted to the standard, but during the time of the experiment the change of temperature of the room altered the resistance slightly; this change was measured and the correction will be plus or minus one-half this. The resistance was adjusted several times during each experiment. The correction is ±D.

Some of the errors which are compensated by the experiment need no remark and I need speak only of the following.

No. 3. By the introduction of commutators at various points all mutual disturbance of instruments could be compensated.

No. 5. In winding wire in a groove, it may be one side or the other of the center. By winding the coils on the center of cylinders which set end to end, on reversing them and taking the mean result, this error is avoided.

No. 6. The circle was always adjusted parallel to the coils of the galvanometer. Should they not be parallel to the needle, G and G″ will be altered in exactly the same ratios and will thus not affect the result. The same may be said of the deflection of the magnet from the magnetic meridian due to torsion.

No. 7. β and δ both ranged over the same portion of the scale and so scale error is partly compensated.

No. 8. The zero-point of all galvanometers was eliminated by equal deflections on opposite sides of the zero-point.

Instruments.

Wire and coils.—The wire used in all instruments was quite small silk-covered copper wire, and was always wound in accurately turned* brass grooves in which a single layer of wire just fitted. The separate layers always had the same number of windings, and the wire was wound so carefully that the coils preserved their proper shape throughout. No paper was used between the layers. As the wire was small, very little distortion was produced at the point where one layer had to rise over the tops of the wires below. Corrections were made for the thickness of the steel tape used to measure the circumference of each layer; also for the sinking of each layer into the spaces between the wires below, seeing that the tape measures the circumference of the tops of the wires. The steel tape was then compared with the standard.

The advantages of small wire over large are many; we know exactly where the current passes; it adapts itself readily to the groove without kinks; it fills up the grooves more uniformly; the connecting wires have less proportional magnetic effect; and lastly, we can get the dimensions more exactly. The size of wire adopted was about No. 22 for most of the instruments.

The mean radius having been computed, the exterior and interior radii are found by addition and subtraction of half the depth of the coil. The sides of the coil were taken as those of the brass groove.

All coils were wound by myself personally to insure uniformity and exactness.

Tangent galvanometer.—This was entirely of brass or bronze, and had a circle about 50 cm. diameter. The needle was 2·7 cm. long and its position was read on a circle 20· cm. diameter, graduated to 15′. The graduated circle was raised so that the aluminium pointer was on a level with it, thus avoiding parallax. The needle and pointer only weighed a gram or two, and rested on a point at the center which was so nicely made that it would make several oscillations within 1° and would come to rest within 1′ or 2′ of the same point every time. I much prefer a point with a *light* needle carefully made to any suspended needle for the tangent galvanometer, especially as a raised circle can then alone be used. The needle was suspended at a distance from any brass which might have been magnetic. There were a series of coils ascending nearly as the numbers 1, 3, 9, 27, 81, 243, whose constants were all known, but only one was used in this experiment. The probable error of a single reading was about ± 1′.

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* To obtain an accurate coil an accurate groove is necessary, seeing that otherwise the wire will be heaped up in certain places. The circle of the tangent galvanometer, which was made to order in Germany, had to be re-turned in this country before use, and much time was lost before finding out the source of the difficulty.

Galvanometer for induction current.—This was a galvanometer on a new plan, especially adapted for the absolute measurement of weak currents. It was entirely of brass, except the wooden base, and was large and heavy, weighing twenty or twenty-five pounds. It could be used with a mirror and scale or as a sine galvanometer. It will be necessary to describe here only those portions which affect the accuracy of the present experiment.

The coils were of the form described above in the theoretical portion, and were wound on a brass cylinder about 8·2 cm. long and 11·6 cm. diameter in two deep grooves about 3· cm. deep and 2·5 cm. wide. The opening in the center for the needle was about 5·5 cm. diameter and the cylinder was split by a saw-cut so as to diminish the damping effect. This coil was mounted on a brass column rising from a graduated circle by which the azimuth of the coil could be determined by two verniers reading to 30″. Through the opening in the coil beneath the needle passed a brass bar 95 cm. long and 2 cm. broad, carrying a small telescope at one end. In the present experiment, this bar was merely used in the comparison of the constant of the instrument with that of another instrument. For this purpose the instrument is used as a sine galvanometer by which a great range can be secured, and it could be compared with a coil having a constant twenty-three times less and which was used with telescope and scale.

The coils contained about five pounds of No. 22 silk-covered copper wire in 1790· turns.

Two needles were used in this galvanometer, each constructed so that its magnetic axis should be invariable; this was accomplished by affixing two thin laminæ of glass-hard steel, to the two sides of a square piece of wood, with their planes vertical. This made a sort of compound magnet very strong for its length, and with a constant magnetic axis. The first needle had a nearly rectangular mirror 2·4 by 1·8 cm. on the sides and ·22 cm. thick. The other needle had a circular mirror 2·05 cm. diameter and about 1 mm. thick. The needle of the first was 1·27 cm. and of the second 1·20 cm. long, and the pieces of wood were about ·45 cm. and ·6 cm. square respectively. The moment of inertia of both was much increased by two small brass weights attached to wires in extension of the magnetic axis, thus extending the needles to a length of 4·9 cm. and 4·2 cm. respectively. The total weights were 5·1 and 5·6 grams and the times of vibration about 7·8 and 11·5 seconds. They were suspended by three single fibers of silk about 48 cm. long.

In front of the needle was a piece of plane-parallel glass. This and the mirrors were made by Steinheil of Munich, and were most perfect in every way.

In the winding of the coils every care was taken, seeing that a small error in so small a coil would produce great relative error. And for this reason the constant was also found by comparison with another coil. The following were the dimensions:

Mean radius 4·3212 c. m.

$$R = 5{\cdot}6212 \qquad r = 3{\cdot}0212$$
$$X = 3{\cdot}475565 \qquad x = {\cdot}935565$$
$$R - r = 2{\cdot}6000 \qquad X - x = 2{\cdot}54000$$
$$N = 1790{\cdot}$$

whence

$$F = 1832{\cdot}25 - 1{\cdot}70\, b^2 Q_2(\theta) - 4{\cdot}50\, b^4 Q_4(\theta) + {\cdot}90\, b^6 Q_6(\theta) - \&c.$$

Taking the mean dimensions of the two needles, we have

$$l = 1{\cdot}23, \quad w = {\cdot}52, \quad n = \tfrac{3}{4}, \quad \cos\theta' = {\cdot}748.$$
$$Q_2(\theta') = +{\cdot}339, \quad Q_4(\theta') = -{\cdot}354, \quad Q_6(\theta') = -{\cdot}275.$$
$$\therefore G = 1832{\cdot}25 - {\cdot}083 + {\cdot}071 - {\cdot}002 + \&c. = 1832{\cdot}24.$$

The coil with which this galvanometer was compared was the large coil of an electro-dynamometer similar to that described in Maxwell's "Electricity," Art. 725, but smaller. The coil was on Helmholtz's principle with a diameter of 27·5 cm., and was very accurately wound on the brass cylinder. There was a total of 240 windings in the coil. The constant of this coil was 78·371 by calculation.

To eliminate the difference of intensity of the earth's magnetism, an observation was first made and then the positions of the instruments were changed so that each occupied exactly the position of the other: the square root of the product of the two results was the true result free from error.

The coils of the galvanometer could be separated so that an outer and inner pair could be used together. By comparing these parts separately and adding the constants together we find G. Hence two comparisons are possible, one with the coils together and the other with them separate. The results were for the ratio of the constants

23·3931 and 23·4008,

which give

$$G = 1833{\cdot}37 \text{ and } 1833{\cdot}98.$$

The mean result is

$$1833{\cdot}67 \pm {\cdot}09,$$

and this includes seven determinations with two reversals of instruments. This result is one part in thirteen hundred greater than found by direct calculation, which is to be accounted for by the small size of the galvanometer coils and the consequent difficulty of their accurate measurement. As comparison with the electro-dynamometer has such a small probable error, and as it is a much larger coil, it seems best to give this number twice the weight of that found by calculation: we thus obtain

$$G = 1833{\cdot}19$$

as the final result.

It does not seem probable that this can be in error more than one part in two or three thousand.

Telescope, scale, &c.—The telescope, mirrors and plane-parallel glass were all from Steinheil in Munich, and left nothing to be desired in this direction, the image of the scale being so perfect that fine scratches on it could be distinguished. The telescope had an aperture of 4 cm. and a magnifying power of 20 was used. The scale was of silvered brass, one meter long and graduated to millimeters.

Induction coils.—A coil was wound in a groove in the center of each of three accurately turned brass cylinders of different lengths. Two of them only were used at a time, by placing them end to end, the ends being ground so that they laid on each other nicely. The two coils could be placed in four positions with respect to each other, in each of which they were very exactly the same distance apart. This distance for each of the four positions, was determined at three parts of the circumference by means of a cathetometer, with microscopic objective, reading to $\frac{1}{75}$ mm. The mean of all twelve determinations was the mean distance. In using the coils they were always used in all four positions. The probable error of each set of twelve readings was ±·001 mm. The data are as follows, naming the coils A, B and C:

Mean radius of A=13·710, of B=13·690, of C=13·720.

Mean distance apart of A and B=6·534, of A and C=9·574, of B and C=11·471.

$N = 154$ for each coil, $\xi = {\cdot}90$, $\eta = {\cdot}84$.

For A and B we have

$$M = 3774860{\cdot} + \tfrac{1}{12}(74250{\cdot} - 66510{\cdot}) = 3775500{\cdot}$$

The remaining terms of the series are practically zero, as was found by dividing one of the coils into parts and calculating the parts separately and adding them.

For A and C

$$M = 2561410{\cdot} + \tfrac{1}{12}(34000{\cdot} - 27230{\cdot}) = 2561974{\cdot}$$

For B and C

$$M = 2050600{\cdot} + \tfrac{1}{12}(27500{\cdot} - 19800{\cdot}) = 2051320{\cdot}$$

The calculation of the elliptic integrals was made by aid of the tables of the Jacobi function, q, given in Bertrand's "Traité de Calcul Integrale" as well as by the expansions in terms of the modulus after transforming them by the Landen substitution.

[To be continued.]

ART. XLVIII.—*On a fourth mass of Meteoric Iron from Augusta County, Virginia;* by J. W. MALLET.

IN 1871 I described[*] three masses of meteoric iron found a few miles from Staunton in this State; still another has lately been brought to light under the following circumstances. About the year 1858 or 1859 a negro man, named Alf, belonging to Mr. Robert Van Lear (on whose land the largest of the three already described meteorites was found), brought to Staunton a lump of iron which he had found, and tried to sell it, but no one considered it curious or valuable enough to pay the price asked, a dollar. This man is dead, and it cannot now be ascertained where he found the specimen, but probably on Mr. Van Lear's land, and undoubtedly in his immediate neighborhood. Failing to sell the mass, Alf threw it away in a vacant plot of ground behind a blacksmith's shop. Here it lay for several years, until it was used, with some other loose material, to build a stone fence. On account of its irregular shape and great weight it soon fell out of the fence, and was then thrown aside in the rear of a dentist's house. He used it for some time as an anvil on which to hammer metals and crack nuts, and afterwards had it built into a wall round the curbing of a cistern. Here, during the summer of 1877, it came under the notice of Mr. M. A. Miller of Staunton,[†] who obtained possession of it, had it removed from the wall, and near the end of the year disposed of it to Messrs. Ward and Howell of Rochester, N. Y. These gentlemen, who were at the time engaged in the arrangement of a geological and zoological collection which they had contracted to furnish for the University of Virginia, allowed me to examine the meteorite before it was sent to Rochester, and have furnished me with material for its analysis. They are having the largest part of the iron cut into slices as specimens for sale.

The shape of the mass is like that of many other metallic meteorites, irregularly rounded, larger at one end than the other, something like a shoulder of mutton in general outline, with well marked concave depressions or pittings. The woodcut on this page is from a photograph of the specimen.

The greatest length was 45·7 centimeters, greatest width 29·2 centimeters, and greatest thickness 20·3 centimeters. The weight was 152 pounds, or 68.950 grams. The crust was not as thick as that upon the masses from the same locality previously examined, and at a number of points the metallic luster of the iron was visible. Magnetic polarity was detectable at various parts of the surface. The specific gravity, taken with a clean piece of 87·5 grams was found = 7·688 at 18° C. The iron is compact and crystalline, with plates of Schreibersite running through it, while a few specks of troilite were detected. On etching with nitric acid the Widmanstättian figures are clearly and beautifully brought out, and their general character is quite the same with that shown upon the etched surfaces of the three previously described masses. On one surface two distinct sets of crystalline markings are observable, the angles of intersection in each of these being nearly uniform.

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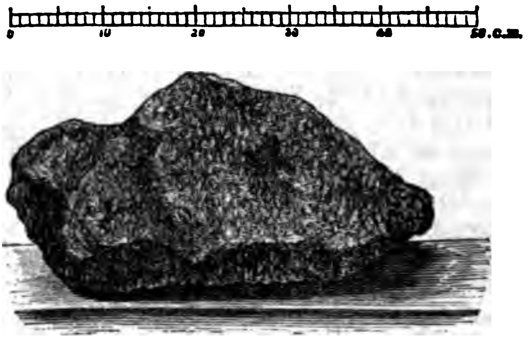
[*] This Journal, III, ii, 10.

[*] The above account of the history of the meteorite was furnished by Mr. Miller.



An analysis made by Mr. J. R. Santos of Guayaquil, Ecuador, now working in this laboratory, gave the following results.

Iron	91·439	Sulphur	·018
Nickel	7·559	Chlorine	*trace*
Cobalt	·608	Carbon	.142
Copper	·021	Silicon (counted as silica)	·108
Tin	*trace*		
Phosphorus	·068		99·963

The chlorine occurs as ferrous chloride, soluble in water. 87·5 grams of iron was used for the analysis, so as to render accurate the determination of the minor constituents. A partial examination of another specimen, however, showed that, as usual in such masses, the distribution of the Schreibersite, and probably of the nickel in the alloy, is not altogether uniform. The average amount of nickel is somewhat less than in the three formerly described masses, and the proportion of cobalt and copper rather larger; but there can be no doubt, I think, that all four specimens, found in the same neighborhood, resembling each other closely in all their physical properties, and exhibiting the same general chemical character, represent different portions of the same meteoric fall.

University of Virginia, March 6, 1878.

ART. XLIX.—*On the Relative Positions of the Forest Bed and associated Drift Formations in Northeastern Iowa*; by W. J. McGEE.

THE term "Forest Bed" was applied by Dr. J. S. Newberry to a "layer of carbonaceous matter, with logs and stumps, and sometimes upright trees,"* observed by him in Ohio during the progress of the geological survey of that State. The same formation has been observed at many points in Illinois, all through southwestern Indiana, at many localities in Wisconsin, throughout northeastern Iowa, in Canada, and in many other places. Wherever found it rests upon true glacial Drift, and within it are found remains of the mammoth, the mastodon, of *Castoroides ohioensis*, *Bison latifrons*, and their contemporaries. In most of the localities just mentioned the Forest Bed is overlaid by a partially stratified deposit, regarding the origin and age of which there is some doubt. The condition of the superficial deposits in the neighborhood of the residence of the writer is such as to throw much light on the question of the true geological position of this formation. A few sections are appended.

I. *A well in Farley, Iowa.*

1.	Surface soil	2 feet.
2.	Clean clay with occasional granite bowlders	15 “
3.	Clay with bowlders, gravel and flint	12 “
4.	Thin-bedded, black, carbonaceous, shaly clay, with fragments of wood	10 “
5.	Thick-bedded do., with much-decomposed fragments of wood. Undisturbed by glacier	4 “
6.	Hard yellow clay, sometimes with bowlders	1 “
7.	Do. with gravel and small bowlders	2 “
8.	Niagara limestone.	

Some of the wood found in this well—probably Willow (*Salix* ——) though not certainly determined—was so well preserved as to be combustible. No. 4 displayed evidence of alluvial action. Fragments of endogenous plants were found in the lower part of this stratum.

II. *A well* 250 *feet from* I.

1.	Surface soil	3 feet.
2.	Clay with occasional granite bowlders	10 “
3.	Clay with bowlders, gravel and flint (broken hornstone), and some sand	8 “
4.	Same as No. 4 in last, but more carbonaceous	7 “
5.	Same as in I	3 “
6. 7.	Yellow clay with sand, gravel and small bowlders	4 “
8.	Niagara limestone.	

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* Ohio Geol. Rep., 1874, pt. I, vol. ii, p. 3.

Pieces of the soft shaly substance from the lower part of No. 4 were found to be slowly combustible, but contained too much earthy matter to burn readily. Charred wood and sticks burned off at one end were found in this well.

III. *A well two miles northwest from last.*

Stratification not personally observed. At about twenty feet a large stump of a tree identified as Red Cedar (*Juniperus virginiana*) was struck in one side of the well. Below it a stratum of the older glacial Drift several feet in thickness was penetrated. The stump retained so much of its organic character as to render the water unfit for use when it rose to its level.

IV. *A well half a mile northeast from last.*

1.	Surface soil	4 feet.
2.	Clay with a granite bowlder weighing 500 pounds	6 "
3.	Clay with gravel	3 "
4.	Hard, dark-blue, shaly clay, with a large piece of wood directly under the bowlder	3 "

V. *A well two miles northeast from last.*

Stratification not observed personally. At about fifty feet fragments of wood, one partially burned, were found intermixed throughout a hard, blackish, laminated clay. Below this the usual yellow clay, with gravel, sand and small bowlders was found resting on Niagara limestone.

VI. *A well quarter of a mile southeast of last.*

1.	Surface accumulation	4 feet.
2.	(Absent, owing to denudation).	
3.	Clay with gravel, sand and bowlders	16 "
4. 5.	Carbonaceous, shaly clay, with fragments of wood, one identified doubtfully as Sumac. Disturbed by glacial and alluvial action	6 "
6. 7.	Yellow clay, with gravel, etc.	4 "
8.	Niagara limestone.	

Farley is twenty-three miles west of the Mississippi at Dubuque, and is about 525 feet higher than that point. The elevation at wells III and V is about the same. IV is about twenty-five feet and VI about fifty feet lower. The general topography beneath the Forest Bed, so far as it has been determined, is not greatly different from that of the present surface. The wells given are but examples of all excavated in the neighborhood, leaving out those in which either the Forest Bed or both it and the older Drift have been removed or modified. The interesting fact is, that the uppermost deposit is in all cases the same, and is beyond the shadow of a oubt glacial Drift very slightly or not at all modified, and xhibiting no distinct stratification. The only difference beween the upper and lower parts is that the lower part conuins a larger proportion of gravel and worn bowlders from ıe immediate vicinity. The upper part contains no bowlders ıdeed except those of granite, syenite, quartzite, and other ıetamorphic rocks from far to the northward. These, howver, are quite abundant. In some fields it has been necessary) remove dozens of bowlders of one hundred pounds weight nd upwards from each acre before the land could be ploughed. ome also are quite large, reaching scores of tons in weight. 'his, too, is a region which our last State Geologist, by some versight, described as being destitute of bowlders.[*] But from n examination of many counties noted for the abundance of ıeir bowlders, the writer has found that these erratics are as bundant throughout a great part of the "bowlderless region" s in any part of Iowa. Glacier-marked bowlders are rare, owever. Perhaps one in a thousand shows plainly grooves nd deep scorings; but many others are less distinctly marked. till not more than one-tenth exhibit any other marks of glacial ction than a rounded form.

The Forest Bed is found at many other localities in Iowa, nd within it the bones of the mastodon and beds of peat ave been discovered.[†] The writer has also seen crania of *ison latifrons* from the same horizon. It has generally been onsidered to be—in other places as well as in Iowa—a postlacial formation. This view finds some support in the characr of the superficial deposits of Ohio, Illinois and Indiana, om the assorted and stratified arrangement of the later Drift rming these deposits. As before stated, however, the formaons in this neighborhood are such as to show conclusively ıat the Forest Bed is overlaid by true glacial Drift, and hence ıust be of interglacial age; and from a recent examination ıe writer is convinced that the overlying deposits in Illinois ; least are formed of glacial Drift deposited since the Forest ed stratum and afterwards modified by alluvial action during ıe slow retreat of the glacier. In Nebraska this carbonaceous ratum has been found resting on glacial Drift and overlaid r both the Drift of the later glacier and the Lœss of the issouri Valley.[‡] The similarity of the organic remains found this stratum wherever exposed indicates a like age for its posits over the whole territory in which it is found.

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[*] Dr. C. A. White's Geol. Rep. (of Iowa), 1870, vol. i, p. 87.

[†] L. c., pp. 117, 118, 119, and 339.

[‡] "Superficial Deposits of Nebraska;" from Hayden's Report for 1874, p. 5.

Farley, Iowa, March 12th, 1878.

ART. L. — *Geographical and Geological Survey of the Rocky Mountain Region under the direction of Professor J. W. Powell. Account of work performed during the year* 1877.

ABOUT the middle of last May, the surveying corps again took the field. This year the rendezvous camp was at Mount Pleasant, a little town in Utah about 125 miles south of Salt Lake City. Three parties were organized under the direction of Professor A. H. Thompson, one to extend the triangulation and two for topographic purposes, the latter being under charge of Mr. W. H. Graves and Mr. J. H. Renshawe respectively, and the former under the immediate direction of Professor Thompson, assisted by Mr. O. D. Wheeler.

The area designated for the season's work lies between 38° and 40° 30′ north latitude, and between 109° 30′ and 112° west longitude, Greenwich, and is embraced in atlas sheets 86 and 75.

Triangulation.—The triangulation party left Mount Pleasant in June. The work of this year being a continuation of the expansion from the Gunnison Base Line measured in 1874, it was desirable to first visit some of the geodetic points established in previous years but the unprecedented amount of snow yet remaining in the high plateaus and mountains rendered this impracticable, and the first part of the season was spent in establishing stations on the Ta-vä-puts Plateau west of the Green River. In midsummer the party was able to visit the high plateaus and connect the work of past years with that of this season. Later the triangulation was extended to the east joining the work of the United States Geological and Geographical Survey of the Territories under charge of Dr. F. V. Hayden and to the north to join the work of the United States Geological Exploration of the 40th Parallel, Clarence King, United States Geologist in charge. The whole area of the season's work embraces something more than 13,000 square miles. The instrument used was the theodolite hereafter described. The points sighted to on the geodetic stations were either artificial monuments or well defined natural points, and all stations were marked by stone cairns.

Topographic Work by Mr. Graves.—The district assigned Mr. Graves for topographic work was the eastern half of atlas sheet 75 and that portion of sheet 86 lying east of the Green and Colorado Rivers, an area of about 10,000 square miles. The most remarkable topographic feature of this region is a bold escarpment facing the south and extending from the western, far beyond the eastern limit of Mr. Graves' work. This is known as the Book Cliffs. At the foot of this escarpment lies a narrow valley through which passes the only practicable route of travel between Central Utah and Western Colorado. South of the valley the whole region is cut by a labyrinth of cañons, formed by the Grand, Green and San Rafael Rivers and their tributaries. This region is one of the most inhospitable and inaccessible in the territory of the United States. It is characterized by extreme aridity, and some portions are cut by narrow gorges, forming "alcove lands." In other portions are found hills of naked sands and clays—regions of bad lands, bold cliffs, towering monuments, hills of drifting, glittering sands and deep tortuous cañons giving to the landscape an appearance strange and weird.

The Book Cliffs rise to an average altitude above their base of 3,000 feet, and about 8,500 feet above the sea-level, and the country from the southern crest inclines gently northward to the valleys of the White and Uinta Rivers. This gigantic terrace, called the Ta-vá-puts Plateau, is cut in twain from north to south by profound gorges through which the Green River runs, known as the cañon of Desolation and Gray Cañon. The drainage of the plateau is northward from the brink of the cliffs through deep narrow cañons for many miles, but at last all these enter the Cañon of Desolation a few miles from its head. North of the Ta-vá-puts Plateau are the valleys of the White and Uinta Rivers. Nearly all the latter and a large portion of the lower course of the former are within the boundaries of Mr. Graves' work.

Over the whole district assigned to Mr. Graves he extended the secondary triangulation. Owing to the peculiar topography of the country, his stations will average about twelve miles apart. He also made a connected plane-table map of the whole area, and complemented his work with orographic sketches.

In the southern portion of the area surveyed by Mr. Graves, considerable bodies of irrigable lands are found along the Grand, Green, San Rafael and Price Rivers; and in the valleys of the Uinta and White Rivers, are other large tracts, on which the waters of the streams named can be conveyed at slight cost. Mr. Graves determined the extent, character and location of these lands, and the amount of water carried by the streams throughout the area embraced in his work.

On the Ta-vá-puts Plateau are small forests of pine and fir, but generally Mr. Graves' district possesses no more timber than sufficient to meet the future local requirements of actual settlers.

Topographic Work by Mr. Renshawe.—The district assigned Mr. Renshawe was the western portion of atlas sheet 75, an area of about 6,000 square miles. The eastern portion of this area is a broad table-land called the Wasatch Plateau, having an average elevation of about 9,500 feet, cut by deep valleys and drained from its very western edge toward the east by the Fremont, San Rafael, Price and Uinta Rivers. The western portion includes broad valleys, abrupt ranges of mountains, and one plateau of considerable extent. The principal valleys in this part are the San Pete, Juab and Utah, all having a general northern and southern trend, an average elevation of about 5,000 feet, and all are drained by the San Pete River and the streams flowing into Utah Lake. The mountain ranges standing between the valleys are the Wasatch, rising in its highest peaks to 12,000 feet, the Lake Mountains and the Tintic Hills each reaching an altitude of nearly 7,500 feet.

The lofty table-land called Gunnison Plateau has an area of about 750 square miles, and an average elevation of 8,000 feet. It is bounded on three sides by almost vertical walls, and is extremely rugged and difficult to traverse.

There is but little irrigable land in the eastern portion of Mr. Renshawe's district, but the broad valleys of the western contain large areas of excellent lands, and the numerous streams furnish a good supply of water.

Mr. Renshawe determined the volume of water in every considerable stream as well as the extent and localities of the irrigable lands throughout his district.

On the plateaus and mountain ranges are large quantities of excellent timber.

On the head waters of the Price River and on Huntington Creek are extensive beds of coal, and on that portion of the Wasatch Range included in Mr. Renshawe's district are deposits of silver and galena.

Mr. Renshawe extended the secondary triangulation over the whole district assigned him, making stations at an average distance of about eight miles, and measuring all the angles of nearly every triangle in the extension. He also made a connected plane-table map of the whole area, and complemented his work with a complete set of orographic sketches.

Hypsometry.—The hypsometric work of this season rests on a primary base established at the general supply and rendezvous camp at Mount Pleasant, and connected by a long series of observations with the station of the United States Signal Service at Salt Lake City. At the base station observations were made with mercurial barometers four times each day, and for eight days during the month, hourly from 7 A. M. to 9 P. M. Mercurial barometers were carried by each field party, and observations made to connect every camp with the base station. All the geodetic points and topographic stations were connected by observations with mercurial barometers either with the camps or directly with the base stations or both. All the topographic stations were also connected with each other by angulation, and from these stations the altitudes of all located points were determined by the latter method.

Instruments. Base-measuring Apparatus.—The apparatus used in measuring the base lines from which the primary triangulation is developed consists essentially of wooden rods aligned and leveled on movable trestles or tripods, the contact being made by coincidence of lines instead of by direct abutment.

The rods are fifteen feet long, one-half of an inch square, thoroughly dried, oiled and varnished. They are supported in cases made truss-form to prevent sagging, and moved in these cases to make the contact by a rack and pinion motion. Either end of a rod is shod with a plate of brass firmly fixed upon and half the width of the rod or one-fourth of an inch, and so arranged, that the plate upon the forward end of any rod projects by the plate upon the rear end of the preceding one, in such a manner that both rods are in the same straight line. The line of coincidence is marked upon both plates and contact is determined by a magnifier. A delicate spirit-level is attached to each case to adjust it horizontally and a thermometer inserted to determine the temperature of the rod. Two steel pins by which the rods are aligned are fixed on the cases directly over the center of the ends of the rods.

The tripods (or stands upon which the cases carrying the rods rest), have short double legs of the usual construction. Firmly fixed upon the tripod heads are two uprights upon which a sliding cross-piece is clamped by thumb screws. Above this cross-piece parallel to and carried with it, is a second which can be moved up or down three-fourths of an inch by means of a long, slender wedge, working between the two cross-pieces and furnishing an easy means of making the final adjustment of the rods in level. The uprights are several inches apart and give sufficient range to all the rods, which is done by a theodolite placed in advance upon the line to be measured. Two or three rods and six or eight tripods are used. The rods are kept in a horizontal position, and when the inequalities of the ground demand, vertical offsets are made with a theodolite. The line is first ranged out and stakes set 500 feet apart along its length, then with six men to work the apparatus, 3,000 feet per day can be measured with all the accuracy the refinements of the triangulation demand.

Theodolite.—The theodolite used in the triangulation is of a new pattern, embracing a number of improvements demanded by the character of the work. So far as possible the number of parts has been reduced by casting in a single piece parts that are usually combined by screws. In this manner the liability to derangement incident to the vicissitudes of mountain work is greatly reduced. The telescope has been enlarged, as compared with the graduated circle, so as to make its defining power bear a greater proportion than usual to the refinement of graduation. The object-glass has an aperture of two inches, and a focal length of twenty. The horizontal circle is ten inches in diameter, and reads by double verniers to five seconds of arc. The vertical circle is five inches in diameter, and reads to one minute. The instrument also embraces other improvements designed to secure greater stability with ease and rapidity in manipulation.

Plane-Table.—In the topographic work the gradientor and sketch book have been superseded by the plane-table and the orograph. The plane-table in use is of a pattern designed by Professor Thompson especially for work of this character. The drawing board is made of a series of slats firmly fixed to canvas in such manner that it can be rolled into small compass for transportation, but when unrolled for work it is so secured by cross-pieces and screws, that great stability is attained. When in use it is fastened to the platen of the orograph. The position of important features in the topography is fixed with an alidade by the usual methods of intersection and resection. Details are placed directly upon the map while they are still under the eye of the topographer, and much of the labor and uncertainty of description by notes is avoided, and the experience of five years in its use has demonstrated that the plane-table, as modified, is equally well adapted to regions of mountains, hills, plains or plateaus. The sketches produced are actual maps and not mere map material. They need only to be adjusted in conformity with the triangulation, and but slight adjustment is necessary. And it has been further demonstrated that a topographer in one field-season can extend his work over an area of about seven thousand square miles, and with all the accuracy necessary for the scale adopted by the Interior Department for the physical atlas of the Rocky Mountain region, that is, a scale of four miles to the inch.

Orograph.—The orograph is an instrument new to topographic surveying, adapted to the requirements of this work by Professor Thompson. It consists essentially of a telescope erected above a platen or drawing-board, on which the movements of its optical axis are recorded. The telescope rotates about a vertical and about a horizontal axis similarly to the telescope of a theodolite, and is connected by simple mechanism with a pencil which rests on a sheet of paper attached to the platen. When the topographer moves the telescope so as to carry its optical axis over the profiles of the landscape, the pencil traces a sketch of the same. This sketch being mechanically produced, is susceptible of measurement, and is a definite and authoritative record of the angular relations of the objects sketched. The instrument is also furnished with a graduated circle on which horizontal angles may be read to the nearest half minute, and this circle is used for the secondary triangulation. The orograph and plane-table are used conjointly, and their results furnish data for the production of contour maps. It is believed that by their introduction the quality of topographic work has been much improved, without addition to its cost. When a topographer takes the field with these two instruments and plane-table sheets on which the primary triangulation has been previously plotted, he returns with a map on which all of the geographic features to be delineated have been determined by their angular relations and the scenic characteristics necessary to give proper effect to the maps, have been outlined by instrumental means. In this manner the subsequent construction of maps at the office ready for the engraver is reduced to a minimum of labor, while for the proper accuracy the topographer is not necessitated to resort to his memory for the appearance of the landscape, but only to the definite record.

Barometers.—The instruments used in the hypsometric work are Green's mercurial mountain barometers, Green's psychrometers and aneroid barometers of the usual construction.

Cartography.—Much attention has been given to this subject for the purpose of determining the best methods of representing the topography of the region surveyed, taking into consideration the character of the country, the more important facts to be embodied, and the scale adopted for the Physical Atlas of the Interior Department. The systems of cartography in use in this country and many of those of Europe have been examined and studied, and many experiments have been made in the office for the purpose of determining the best methods adapted to these circumstances and conditions.

For the Physical Atlas heretofore mentioned, it is proposed to represent the topography by contour lines with auxiliary hatchings to indicate rock surface, and shading for general reliefs, these so applied as not to obscure the contours. For special purposes hatched maps are used, for others contour maps, and for purposes relating to the discussion of geological structure, maps are made by photographing or lithographing models or relief maps in plaster.

Classification of Lands by Mr. Gilbert.—The Survey under the direction of Professor Powell has been extended over the northern portion of Arizona and the greater part of Utah, but a broad strip along the northern end of the latter Territory was embraced in the survey made by Mr. Clarence King, under the War Department. It seemed desirable, however, to extend the classification of lands over this latter region, and this duty was assigned to Mr. G. K. Gilbert.

Mr. Gilbert took the field at Salt Lake City, and traversed all of the Territory lying west, north and northeast of that point, a tract comprising so much of the drainage basin of Great Salt Lake as lies in Utah. In this area is included the most valuable portion of the Territory, as well as one of the most sterile. A very small part of it will repay cultivation without irrigation, but this is exceptional, and in general the possibility of agriculture depends upon the possibility of artificial watering. The Bear River, the Weber, and the Jordan carry as much water as can profitably be used upon all the lands to which it is practicable to convey them by canals, and these lands were measured in order to determine the agricultural capacities of their valleys. The smaller streams, on the contrary, are inadequate to serve the arable lands through which they severally run, and their agricultural capacities were ascertained by measuring the volume of each stream. East of Great Salt Lake are great mountain ranges, the Wasatch and the Uinta, and large streams flow from their melting snows all through the summer season. The Bear, the Weber, and the Jordan flow to the lake, and the three rivers can be made to reclaim 800,000 acres of land in their valleys. This is twelve and one-half per cent of the district that they drain. West of the lake the plains are interrupted by mountains of great magnitude, the snows of winter are dissipated too early in the spring to be of use for irrigation, and much of the land is an absolute desert. In a total area of 8,300,000 acres only 21,000 acres are of value for farming—one-fourth of one per cent.

These estimates are based upon the experience of the farmers of the district, who have practiced irrigation for thirty years, and have given it a greater development than can be found elsewhere in the United States. They have now under cultivation a third part of the irrigable lands of the Salt Lake Basin, and are utilizing many of the small streams to the full extent of their capacities. A careful study was made of their operations, for the purpose of learning the quantity of water necessary to redeem a given quantity of land under various conditions of soil and climate, and the resulting determinations were used in computing the areas susceptible of irrigation by the streams and parts of streams unused. The greater part of the future extension of the cultivated areas will be accomplished only by expensive engineering works, including the damming of the principal rivers and the construction of long canals. Five million dollars is probably a moderate estimate of the cost of redeeming the 500,000 acres that are susceptible of reclamation, and the requisite capital will have to be concentrated upon a small number of large canals.

Since the first settlement of the territory in the year 1847 the water supply has increased. It is reported by the citizens that each stream is now capable of irrigating a greater area of land than when it was first used. Creeks that once scantily watered a few acres of ground now afford an ample supply for double, treble, and even fifty times the original area. This increase has been accompanied by a rise of Great Salt Lake, which having no escape for its water except by evaporation has stored up the surplus from the streams.

For the purpose of investigating the extent and the cause of the increase of the streams, Mr. Gilbert made a study of the fluctuations of the lake. It was a matter of common report that the surface of the water had been subject to considerable changes and that on the whole it had greatly risen since its shores were first settled; but previous to the year 1875 no systematic record of its movements had been kept. In that year a series of observations was inaugurated by Dr. John R. Park of Salt Lake City, at the suggestion and request of the Secretary of the Smithsonian Institution. A small pillar of granite, graduated to feet and inches, was erected at the water's edge near a rocky islet known as Black Rock. The locality was then a popular pleasure resort, and the record was undertaken by Mr. J. T. Mitchell. Observations were made at frequent intervals for more than a year, but were then interrupted by reason of the disuse of the locality as a place of resort, and they have not since been resumed in a systematic way. To obviate a similar difficulty in the future, Mr. Gilbert caused a new record post to be established near the town of Farmington, where the work of observation has been undertaken by Mr. Jacob Miller, and it is anticipated that in the future there will be no break in the continuity of the record.

In the interval from 1847 to 1875, during which no direct observations were made, there was nevertheless a considerable amount of indirect observation incidental to the pursuits of the citizens. The islands of the lake were used for pasturage, and the facilities for the transfer of cattle to and fro were greatly affected by the fluctuations of the water. A large share of the communication was by boat, and the frequent changes of landing place which the boatmen were compelled to make impressed upon their memories the character and order of the principal oscillations. In pursuance of the inquiries instituted by the Secretary of the Smithsonian Institution, the testimony of the boatmen was compiled by Mr. Jacob Miller and a history of the oscillations was deduced.

A similar and corroborative history has been derived by Mr. Gilbert from an independent investigation. Two of the islands used for pasturage are joined to the main land by broad, flat bars, and during the lower stages of the lake these bars being either dry or covered by a moderate depth of water, have afforded means of communication. It happens that the Antelope Island bar was in use until 1865, when it became so deeply covered that fording on horseback was impracticable; and that the Stansbury Island bar was first covered with water in 1866, and has been used as a ford, with slight exception, ever since. By the compilation of the testimony of those who have made use of these crossings, a continuous record was derived, which cannot deviate very widely from the truth, and the work was checked by making careful soundings to ascertain the present depth of water on the Antelope Island bar.

From 1847 to 1850 there was little change beside the annual tide variation dependent upon the spring floods, and which makes the summer stage in each year from one to two feet higher than the winter. Then the water began to rise and so continued until in 1855 and 1856 its mean stage was four feet higher than in 1850. This progressive rise was followed by a progressive fall of equal amount, and in 1860 the lake had returned to its first observed level. In 1862 there began a second rise which continued for eight years and carried the water ten feet above the original level. Since 1869 there has been no great change, but the mean height has fluctuated through a range of about two feet.

As the lake has risen it has encroached upon the land, and the shores are in many places so flat that large areas have been submerged. At one point the water edge has advanced fifteen miles, and the surveys of Captain Howard Stansbury and Mr. Clarence King show that from 1850 to 1869 the total area of the water surface increased from 1,750 to 2,166 square miles, or nearly twenty-four per cent. By this expansion the surface for evaporation was increased so that the lake could return to the atmosphere the surplus thrown into it by the augmented streams.

Whatever land is at any time flooded by the lake becomes saturated with salt, and if the water afterward retires, remains barren of vegetation for many years. The highest level reached by the water in storms is marked by a line of driftwood and other débris. Above this line there is usually a growth of grass and sage brush, but below it nothing grows. Previous to the last great rise of the lake, the storm line was six feet lower than at present, and the intervening belt of land still retains the stumps and roots of bushes that have been killed by the advancing brine. The encroaching water overran the ancient storm line in about the year 1866, and for the past eleven years it has covered ground which had been exempt from incursions of brine for a time sufficient to permit the rains to cleanse the soil, and for a further time sufficient to produce a growth of sage brush. The whole period is as likely to have been measured by centuries as by decades.

Thus it appears that the last twelve years have witnessed an extension of the lake, which is not only without precedent in the experience of the citizens of Utah but is clearly an anomaly in the history of the lake. To explain it and to explain at the same time the increase of the streams, there are two general theories worthy of consideration.

The first is that there has been a change of climate in Utah whereby the atmosphere is moister, so that the fall of rain and snow has become greater and the rate of evaporation has become slower. The second is that the industries of the white man, which have been steadily growing in importance for the last thirty years, have so modified the surface of the land that a larger share of the snow and rain finds its way into the water-courses and a smaller share is returned to the air by evaporation from the ground. The latter theory, which is the one proposed by Professor Powell, is considered by Mr. Gilbert the more probable, and he finds reason to believe that the tax imposed upon the streams by the work of irrigation is more than repaid by the effects of the draining of marshes and the destruction of herbage and timber. A great volume of water is turned upon the cultivated fields, and from their moist surfaces is absorbed by the atmosphere without ever reaching the lake; but on the other hand the farmer has found it to his advantage to drain the beaver ponds and other marshes and thus check the evaporation from their surfaces, and the streams which he thereby rescues from dissipation are used in irrigation for a few months only, while for the remainder of the year they pay their tribute to the lake. The destruction of grasses by herds of domestic animals and the cutting of trees upon the mountains expose the ground to the sun and facilitate the melting of the snow; the removal of the grass opens the way also to a freer circulation of surface waters, so that the rain and melting snows are gathered more quickly and thoroughly into rills and streams; and both these influences increase the inflow of the lake.

This discussion has an important bearing upon the agriculture of the arid region, for if the theory favored by these gentlemen is the true one, the work of irrigation can be pushed forward with the confident assurance that the supply of water is more likely to increase than diminish in the future; and it may even be possible when the subject has been fully developed, to devise measures which shall directly promote the increase.

Geological work by Mr. Gilbert.—During the preceding summer Mr. Gilbert had discovered a peculiar series of phenomena produced by recent orographic displacements, and he has this year found opportunity to study them in numerous new localities. It appears that the system of faults and flexures—the system of upward and downward movements—by which the mountain ranges and the valleys of Utah and Nevada were produced have continued down to the present time. Evidence of recent movement has been discovered on the lines of many ancient faults. The ancient shore line of Great Salt Lake which is exhibited so conspicuously upon the surrounding mountain slopes, and which must have originally been level, is no longer so, but has been shifted up and down by the displacement of the mountains. Its present altitude above Great Salt Lake was determined at four different points by spirit-level; and the determinations were found to range from 966 feet to 1,059 feet. The measurements by level were all made in the immediate vicinity of the lake, but the barometer indicates that at points more remote the discrepancy is greater.

These observations are valuable additions to our evidence that mountain-making is a work of the present as well as of past ages, and that the grand displacements by faults and folds are caused by slow intermittent movements.

Mr. Gilbert also traced and mapped the northern portion of the ancient shore line of the Lake from Salt Lake City to Redding Spring, following its sinuous course for 900 miles in Utah, Idaho and Nevada, and demonstrating that the ancient outlet he had discovered the preceding summer at Red Rock Pass was the only one by which the lake had ever discharged its water to the Snake River.

During the winter of 1876–77, Mr. Gilbert prepared his Report on the Geology of the Henry Mountains, and the manuscript was sent to the printer.

The Henry Mountains constitute a small group in Southeastern Utah and stand quite by themselves. They are of a peculiar character and represent a type of structure that has never before been fully described. Mr. Gilbert's report is a monograph at once of the mountain group and of the type of mountain structure. The mountains are of igneous origin, but the rising lavas, instead of outpouring at the surface of the earth, in the usual way, failed to penetrate the upper portions of the crust and formed subterranean lakes or chambers. The strata lying above the lava lakes were upbent in the form of great bubbles, and from these bubbles of sandstone and shale with their cores of trap, the erosive agents of the air have carved the mountains. The mountain structure is thus twofold, comprising first volcanic upheaval, and second atmospheric and aqueous degradation. To aid in the discussion of the first element of structure, Mr. Gilbert constructed a stereogram of the district in plaster, exhibiting the forms due to upheaval as they would appear if unmodified by degradation. He prepared also a topographic model, exhibiting the same forms as actually modified; and the two models will be reproduced by photography to illustrate the report. The treatment of the second element of structure is of a thorough character and includes a discussion of the general principles which control the sculpture of the land surfaces of the earth by rains and rivers. The volume is ready for the binder.

Geological work by Captain Dutton.—Captain Dutton resumed his exploration of the same field which he has been studying for three years, having recognized in it a certain unity which renders it eminently adapted to an important monograph. The region explored by him is centrally situated in the territory of Utah, extending from Nebo in the Wasatch nearly southward a distance of about 180 miles and having a maximum breadth of about 60 miles. It possesses certain features which serve to distinguish it both topographically and geologically, and he proposes to call it the District of the High Plateaus of Utah. It consists of a group of uplifts now standing at altitudes between 9,000 and 11,500 feet above sea-level, while the general platform of the country is from 5,000 to 7,000 feet high. The plateaus have been carved out of this platform by great faults, and the general structure corresponds closely to that described by Professor Powell under the name of the Kaibab structure and illustrated by him in his section of the region traversed by the Grand Cañon of the Colorado. The relations of this belt of high plateaus to the regions adjoining are of special interest. At the close of the Cretaceous, the country lying to the eastward of it passed by gradation from an oceanic to a lacustrine condition, the intermediate stage presenting doubtless a strict analogy to the condition of the Baltic. This Eocene lake area now constitutes the southern part of the drainage system of the Colorado River. During Cretaceous and Eocene time the area now occupied by the Great Basin was dry land, and its denudation must have furnished a large part of the sediments which were spread over the bottom of the great lake. The movements, which took place during the Eocene, at last resulted in the desiccation of the lake, and though a strict chronological correlation to European and other divisions of time cannot be made with certainty, it may be provisionally inferred that this desiccation was completed before the commencement of the Miocene. It was brought about by the more rapid uplifting of the lake area than that of the Great Basin until at last the former area became the loftier of the two, thus reversing their relative altitudes. The lake area is now a portion of the so-called Plateau Country and since the commencement of the Miocene (para-Miocene) has been subject to a great and continuous erosion. The district of the High Plateaus occupies a portion of a narrow belt separating the Plateau Country from the Basin Province and therefore stands upon the locus of the ancient shore line which in the lacustrine stage bounded the two areas. To that shore line they stand in an intimate and remarkable relation. To its trend the great displacements maintain not merely general parallelism but an approximation to strict parallelism both in totality and in detail which would not have been anticipated and which cannot be purely accidental, and seems to point to some definite determinative association between the littoral deposits and the great lines of displacement. The great structural features are these faults and their equivalent monoclinal flexures. They are remarkably persistent, extending in parallel courses throughout the entire length of the region surveyed. One of them, the great Sevier fault, becoming here and there a monoclinal flexure, has been traced continuously over a length of 240 miles, and others of nearly equal persistence have been noted. The High Plateaus belong to the Plateau Country, for notwithstanding the great amount of dynamical energy displayed in their uplifting they preserve in a remarkable manner the plateau type of structure, which is distinguished sharply from the arched, flexed and tilted types prevailing in other disturbed localities. There is an abrupt transition from this plateau structure to that found in the adjoining Basin. There are some localities where one may hurl a stone from one province to the other, and in general it may be said that the dividing line must pass within a single range or table. The Plateau Province seems to stand here in strict correlation to the Tertiary beds; where the tertiaries end there also end the plateaus. The relation between the Tertiary and Cretaceous throughout this belt is one of general unconformity; in many places where the contacts are seen the Tertiary is revealed lying across the upturned and eroded edges of the Cretaceous, showing clearly a break between those portions of the two series which are here preserved.

But of all the features displayed by the High Plateaus the most remarkable are the manifestations of former volcanic activity. Both in area and thickness the volcanic emanations are very extensive. They cover more than 5,000 square miles, and sections of 4,000 to 5,000 feet are presented without revealing the lowest beds. The greater part of the eruptions took place after the lake basin had been drained or had shrunken to limits outside of the district, for sedimentary beds have not been found intercalated between the various flows but always underlie them. It is therefore impossible to fix with great precision the commencement of the outbreaks, but the general indications are, that they began very soon after the close of the lacustrine period, and they may have commenced still earlier. The eruptive epoch was undoubtedly a long one. The individual flows are very numerous and represent all the great groups of eruptive rocks. In many cases the quantity of material extravasated is so great that the eruptions may well be called massive, not however of such marvellous extent as asserted to have been poured forth from fissures during the Basaltic period in Oregon and Northern California; but there are many individual sheets which surpass in magnitude any which is known to have emanated during recent or modern times from any existing volcanic vent at a single eruption. From what openings these masses were extravasated it is usually difficult to fix with certainty and precision, so vast are the accumulations and so expansive are the sheets, and at the same time so numerous that wheresoever they were emitted the earlier vents must have been buried by later deluges of lava, and even the more recent vents, except in the case of the latest basalts, have been swept away by slow erosions in the long period which has elapsed since their activity was extinguished. There are, however, still remaining, distinct traces of localization of eruptive activity in the form of greater accumulations, at some points, from which in most directions the total thickness of the volcanic series appears to attenuate. Moreover in those central localities of maximum accumulations there appears to be a large amount of what might be called in a certain sense unconformity of the various eruptions, and greater irregularities in their bedding as compared with the more even layers and more regular distribution of the sheets more remote from these centers. This fact appears to be of general application also to existing volcanic regions of great extent. Captain Dutton has succeeded in locating at least five areas from which the various overflows appear to have emanated, and believes that further research might result in the determination of many others.

At the time these eruptions were in progress it is probable that the country was not an elevated one as at present, though it might have been a rising area. The great displacements consisting of immense faults of extraordinary length and persistency took place after the close or during the decadence of the principal eruptions, and it was at this latter epoch that the greater part of the general elevation of this portion of the Plateau Province occurred. During its progress many eruptions must have taken place, and their later age is readily identifiable, but none of them were comparable in extent and in the volume of ejected materials with older eruptions prior to the great displacements. Although it is ordinarily not difficult to determine whether a particular event preceded or followed some other event within the locality, there seems to be no way of correlating these different events strictly with the epochs which are designated by the sedimentary formations of the adjoining country, and it is therefore impossible to determine the exact period in the chronological scale, at which the faulting took place, further than the fact that it must have occurred long enough after the close of the Eocene to allow for the accumulation of these vast bodies of volcanic beds. This may carry the period of faulting far into the Miocene period, or possibly as far as the commencement of the Pliocene. But while the first stage in the activity of these ancient volcanoes was undoubtedly the greatest and accompanied by an incomparably greater amount of extravasation, it by no means constitutes the the whole of it. Even after the great displacements and after the principal topographic features of the country depending upon structure as they now exist had received their shape, minor eruptions continued. They present, however, a somewhat remarkable fact. The later eruptions did not take place from the same centers as the earlier ones, but show a tendency to recede from them and to occur around the borders of the older volcanic district. The central portions of the volcanic area are unquestionably the oldest, while the younger ones are found around their borders and sometimes at considerable distances.

One point which during the study of this region has engaged the careful attention of Captain Dutton, has been to ascertain whether it presents any such sequence in the lithological character of the eruptions as is asserted by Baron Richthofen to prevail in the volcanic districts of Europe, South America, Asia Minor, and the Sierra Nevada. This asserted sequence has engaged the profound attention of most vulcanologists and is of great importance in relation to all questions bearing upon the origin and causes of volcanic action. Although at first disposed to doubt the prevalence of this sequence, and not favorably impressed with the speculations and theoretical views of Baron Richthofen, Captain Dutton has reached the conclusion that the high plateaus of Utah exhibit in a decided manner essentially the same sequence which Richthofen claims for other volcanic regions. The earliest eruptions consist of rocks agreeing well in lithological characteristics with those described by Richthofen under the name of propylite. This rock is usually concealed, if it exists in any great quantities, by the later flows, but is in several places brought to light partly by the great displacements and partly through the agency of erosion, and wherever found it is seen to occupy the lowest position of all. It is also worthy of note that this rock is found at those points which constitute the centers of eruption before referred to, showing that the activity which it ushered in continued to have its seat through a long cycle in and about the same locality. The propylite is succeeded by a rock answering to Richthofen's description of hornblende andesite, which is usually overlaid by a rock rich in augite with triclinic feldspar, which may be termed augitic andesite. Still higher in the series are found immense masses of trachyte which, however, is frequently intercalated with dolerite. The variety of the trachytes is very great—so great indeed that were it not for the persistence of certain mineralogical as well as textural characteristics, which are universally accepted as being distinctive of that group of rocks, one might feel strongly tempted to make numerous subdivisions of them. The extremes of the varieties of the trachytes might be represented at one end by a coarsely granular micaceous rock composed chiefly of orthoclase, sometimes hornblendic and sometimes augitic; at the end is a highly porphyritic trachyte, consisting of well-developed orthoclase feldspar imbedded in a fine paste highly charged with peroxide of iron. Between these masses of trachyte are intercalated, though in subordinate quantities, beds of dolerite, showing distinct crystals of striated feldspar, with great abundance of augite and magnetite. In the earlier and grander periods of the eruptions the following sequences may therefore be recognized: first, propylite; second, andesite; and third, interblended trachyte and dolerite. Still later than these and occurring at new centers of eruption were outpours of rhyolite, while last of all were erupted around the outskirts of the district great quantities of true basalt. There does not appear to be any single locality where all the groups of rocks are found superposed; nevertheless the relative ages do not admit of any doubt, whether the beds are superposed or not; but while furnishing a general verification of the sequence, the district of the High Plateaus presents the fact with certain modifications which may be set forth in the following manner. In the lithological scale, propylite and hornblendic andesite are very nearly intermediate between the extremes of acid rocks represented by rhyolite, and basic rocks represented by basalt. Taking propylite as a starting point in the scale of classification, we find two divergent series proceeding on the one hand toward the acid end of the scale, and on the other toward the basic end. As we follow the eruptions down into the later epochs we find that both series are represented in a certain sense independently of each other, so that they intercalate; the acid becoming at one end more acid with the progress of the volcanic cycle, and the basic rocks becoming more basic. Each series seems to pursue its own order and to be subject to its own law, so that being originally divergent, they become more and more widely separated in their lithological characters, as the cycle proceeds. Thus at the commencement of the activity we have propylite and hornblendic andesite, which are closely assimilated to each other in their physical characteristics; at the middle stage we have trachytes and moderately basic dolerites, which are moderately separated, and at the close we have rhyolites and basalts, which stand at the opposite ends of the scale.

Plan of Publication.—In the geological branch of the work, the plan has been adopted, as far as possible, of publishing monographs, each embracing all the studies made by the corps of any particular region to which it relates, preferring this to a system of annual reports consisting of *résumés* of the field notes of each season. In the preparation of these monographs, relief maps or plates are constructed on a scale of two miles to the inch or larger, vertical and horizontal scales being equal, and to correspond with each relief map a stereogram in plaster is constructed on the same scale, designed to exhibit such surface as would appear had there been no degradation by atmospheric agencies, but displacement can be studied independently of the phenomena of degradation, with which in nature they are always associated, and by which they are more or less obscured; and a comparison of the stereogram with the relief map gives approximate quantitative results of degradation; that is, the two factors of mountain structure, elevation by displacement and degradation by rains and rivers are separated, that each may be considered independent of the complicating conditions of the other.

Geological Illustration.—Much attention has been paid to the graphic representation of the important features of geological structure. The Rocky Mountain region has proved to be one of great interest in this branch of investigation, because of the peculiar features of its physical geography. Long and towering escarpments are found, deep cañons with precipitous walls are numerous, its hills and mountains are often without soil and vegetation, accumulations of sub-ærial or glacial drift are infrequent, and thus the general rock-structure is well revealed. Several new methods of illustration have been devised, some of which have already appeared in the publications of the Survey.

[To be continued.]

ART. LI.—*Just Intonation in Music. Its Notation and Instruments;* by HENRY WARD POOLE, M.A., of South Danvers, Massachusetts; and Professor in the National College of Mexico.

IT is evident that there is a general want of intelligence concerning the fundamental laws of Musical Intonation, the constitution of the so-called scale and the means of attaining practically just melodies and harmonies. There is doubt still even among those most eminent in acoustics as to whether the *septimal* ratios are admissible in practice. Others who would be glad to have it used, are doubtful whether the nice distinctions of comma, etc., can be recognized and sung. Without reference to previous times, I can declare from abundant personal knowledge, and actual demonstration, that the chord of the seventh is most agreeable and easy to give with the just ratio of 4:7, and that there is no practical difficulty in singing in just intervals. There appears to be a general desire expressed for such perfect intonation, and in consequence the following observations may be interesting.

However complicated this subject may appear when studied in books which do not have any primary definitions or canons, it can be made as clear and orderly as naturally it would be supposed, in view of the fact that music is based upon the mathematics.

Classification of Musical Tones into three Principal Orders.—Admitting into music only the prime numbers 2, 3, 5, 7, and considering the 2 as auxiliary, forming octaves, and inversions, but not producing new or original harmonies, there remain three others, which give the *Fifth*, 2:3, the *Third* (major always, unless otherwise expressed), 4:5, and the *Seventh* (Perfect or Harmonic), 4:7. A *series* (each a fifth from the next,) is formed of each of these kinds, and we have three Prime Series. A due mixture of the notes of each is necessary for harmony. Using the letters, C, D, E, etc., we may express the series to which each belongs in various ways. In common notation it is known to the intelligent singer or violinist by his perceiving the relation of each note in the scale. All the strings of the violin, violincello, etc., are tuned to the First series, that of Fifths. The open strings of the violin sound justly only in the key of D. In G they are equally true except the highest, which is a comma too high for the third of C, which is in tune with the open G. In other words, there are two tones with the same name. We have distinguished these by E, the higher, and e, the lower. For greater clearness in writing down the scale, and in its analysis and vocal practice we will color the E, and all of the First series with *Red:* giving to the e, and all of the Second series, the *Yellow*, which, to show well by artificial light will be well done by gold. In like manner, the Third series, that of the Sevenths, indicated by a gothic letter, will be colored *Blue.** Nothing remains but to call the tones by distinctive names. The syllables of Guido answered this purpose when there was no "modulation within the key," and no account made of Perfect sevenths. To the modern syllables belong two, which were not in the original in the "*Ut queant laxis;*" to wit, the "Do," and the "Si." Keeping the Do as it is, and changing the initial letter of the si, so as not to confuse it hereafter with that of sol, we may find a common ending to all the notes of the first series, or that of the Fifths. These are the 1st, 2d, 4th and 5th. Preserving the 1st and 5th as they are, but rejecting the final letter of the sol, and making the rest conform, we shall have Do, Ro, Fo and So. Their *class* is known by the termination; the *place* of each in the scale by the initial consonant. For the second class, that of the Thirds, mi, la, si, we preserve also the present termination of the majority, and have mi, li, and zi. For the Perfect sevenths or Third series, which are generally sounded by the termination of e (Italian), we preserve the same, and zi flat is **Ze**. And the dominant seventh, lower than Fa, will be **Fe**. There are two different Diatonic Scales according as the Seventh 4 : 7, is introduced or not; they will be expressed in reference to their syllables for singing, their letters, colors, relative vibrations and intervals, as follows:

TRIPLE DIATONIC SCALE.

Common Chords on C, G and F.

	Do	Ro	mi	Fo	So	li	zi	Do
In key of C,	C	D	e	F	G	a	b	C
Colors,	Red	Red	Yellow	Red	Red	Yellow	Yellow	Red
Relative Vibrations,	48	54	60	64	72	80	90	96
Intervals,		8 : 9	9 : 10	15 : 16	8 : 9	9 : 10	8 : 9	15 : 16

DOUBLE DIATONIC SCALE.

Common Chord on C; Chord of Seventh (and 9th) on G.

	Fo	So	li	**Ze**	Do	Ro	mi	Fo
Key of C (Tonic F),	F	G	a	$B^\flat$	C	D	e	F
Colors,	Red	Red	Yellow	Blue	Red	Red	Yellow	Red
Relative Vibrations,	32	36	40	42	48	54	60	64
or	48	54	60	63	72	81	90	96
Intervals,		8 : 9	9 : 10	20 : 21	7 : 8	8 : 9	9 : 10	15 : 16

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* I had selected for the three Prime Chords, the colors generally considered as Prime; applying to the Green, which has optical claims against the Yellow, the test of the proportional rate of vibration, I find that the *Green really corresponds*, as nearly as could be expected in such a determination, *with the Prime Chord of the Third.* The Red, Yellow, Green, and Blue being as Do, Ro, mi and **Fe** (the dominant 7th), or as the numbers of the Double Diatonic Scale 32, 36, 40, 42, the Violet approximating to the So or 5th. The Yellow in all the tables comes very near to Red as 9 : 8. The Orange does not appear to have any correspondence in the acoustic scale.

The Double Diatonic Scale has two notes differing from two of the triple scale on the same tonic—which is F; although its tones are all in the key or scale of C, being identical with the Triple Diatonic of C, with the exception of the change of b into **B**♭. But its ending and tonic is F. Having thus provided for the notation and distinction of these different series of Primary Tones, and using the colors, we shall find on trial by the ear, or by examination of the music in use, especially when written as here indicated, that at least two different series (or colors) must appear in every chord, or it is wanting in *richness* or effective *ring.* Especially in duets is this observed; the two voices are constantly giving pairs of different colors. The duplicates or notes of the remaining series—there being three—are given by the accompaniment, if there be any. Two hunting horns having to limit themselves to what might be called a "Single Diatonic Scale" (as having but one chord, say of C, with seventh, ninth, etc.,) and making their own fundamental harmony, cannot comply fully with this rule; they give at times fifths, (both red) without any third. The necessity of pure elements in these nice combinations is soon perceived.

For an example in point we may take the triple (or common) diatonic scale, giving to one voice this scale and to another the accompaniment; noticing by the difference of type that each pair is of different series. *Note.*—The 2d and 4th can never accord (D, F, 27 : 32). *Rule.—Tones a third apart*—two degrees—*must be of different series.* (Sixths, being only an inversion of thirds, are subject to the same law.) *Note.*—D, **F**, are harmonious, 6 : 7, and d, F, 5 : 6, are also good: the d is the "grave second" of the old theorists, and is the sixth of the scale of F, and occurs in the natural key in cadences and "modulations within the key."

Scale of C, harmonized.

C D e F G a b C	C b a G **F** e D C
unison b C a e F G e	e G **F** e d C b unison

In descending, the F is accompanied by this grave d with natural and easy effect. As this momentary inclination to the subdominant chords is made, so the double diatonic enters, and instead of F, G, e, at close of the ascending scale, we could have F, **F**, e—by syllables, Fo, **Fe**, mi. But never will be seen a chord of the Third (or Sixth) from the same series.

The instruments of just intonation are those of free tones, as the voice, the violin class, and the trombones, etc., and those of fixed tones, as the organ and other instruments to be described.

The Enharmonic Key-board for organs, etc., described in this Journal in July, 1867, while preserving its essential principles, is capable of being varied to embrace more or less series of sounds, and to give to each the prominence desired. I shall describe one which embraces the three series of Prime chords, and, in addition, a Fourth, of the *leading notes to the Thirds* of Series II, which are used in ornamental passages and the "minor mode." The plan is formed by dividing the octave—$6\frac{1}{2}$ inches—into 24 equal parts; the distance of nine-tenths of an inch being laid off from front to back, 14 times, will provide for the tones from C flat to F sharp. All the divisions for the digitals will fall upon these lines, and will be understood by reference to the diagram, the mutual relation being the same in all parts of the boards. The digitals of each signature are elevated one-tenth of an inch as they go backward. The base of the digital C, with its 3d, e, and *d*♯, (leading note to e,) and its seventh $B^{\flat}$ are $\frac{1}{10}$ of an inch higher than F, a, *g*♯ and $E^{\flat}$, and *the same* distance lower than G, b, a♯ and F. The white keys

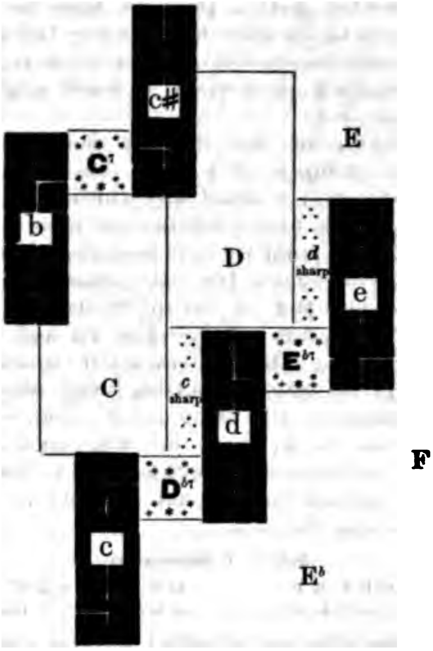

Diagram.—Simply to show the *relative position* of the Digitals of the four Series; their exact sizes being as follows:

$a = 6{\cdot}5$ inches + 24 ($= 0{\cdot}27$). $b = 0{\cdot}9$.

Series.	Width.	Length.
I. (C, D)	$4a$ (and $3a$)	$4b$
II. (c, d, e)	$2a$	$3b$
III. (C^7, $E^{\flat 7}$)	$2a$	$1b$
IV. (*c*♯, *d*♯)	$1a$	$2b$

of Series I being at this level, the black keys rise 0·45; the orange keys, (Series IV,) are 0·75; and the blue keys of Series III are 0·90. All the elevated keys are reduced in width, down to the base, so as to allow the finger to enter freely between them. The number of digitals being 48, each lever is taken at the half of these primary 24 divisions, and all lie in one level at the rear of the key-board. In construction, the number of pipes (or *vibrators*, in the cabinet organs) is reduced by using the same for those which being at the distance of 8 Fifths and 1 Third are practically identical. (§ 29, 30 this Journal, July, 1867).

Justly-Intoned Pianofortes.—It is desirable to obtain a loud and full tone by a single wire, large and at full tension. Then with an enharmonic key-board and the triple sets of wires, the whole will be easily kept in tune, and will sound louder than if they were tempered.

Wind Instruments of the class of the horn, cornet, etc., depend, for the fundamental tone, on the length of the tube, and for the harmonics, on the tension of the lips. The fundamental length may be varied by the common cornet valves or "pistons" and corresponding supplementary tubes to give the tones of series I. These valves are arranged in the order of the fifths, or thus: F, C, G, D, A, E, B, etc. To play the triple diatonic scale of C will be used the valve of C (or the simple tube of the whole instrument, if so constructed) and those of F and G, on its right and left; the order of these dominants, etc., is the same in every key. For the sake of economy in construction, or to save weight, while playing, there might be four or five valves, and the player before beginning might select the tubes he is going to need, as is done with the simple horn, which can have but one at a time. Any one can try the effect of this system with a common cornet and a pair of tubes —india-rubber will do—which will add to the fundamental tube 50 and 33 per cent for the subdominant and dominant.

The *Trombones* can be rendered not only capable of just intonation, as they now are, but with fixed and certain tones; this can be done in various ways, including a combination of the sliding double tube to fix the tonic, with three valves with their tubes kept in due relation with the tonic—varying as the whole tube is longer or shorter—so as to give dominant and subdominant to any tonic. A change of key is thus made by simply moving the trombone slide to the new key-note.

It is easier to play these enharmonic instruments, than the common tempered ones; the director can mark the changes to be made. Joined with the violins and voices excellent effects will be produced. The clarionets, etc., can be favored by good players so as to fall in with the rest in the just and perfect concert.

ART. LII.—*On certain Forms of Brachiopoda occurring in the Swedish Primordial;* by S. W. FORD.

IN a paper "On the Brachiopoda of the Paradoxides beds of Sweden," published in the Transactions of the Royal Swedish Academy of Sciences, for 1875, M. G. Linnarsson institutes the genus *Acrothele* for the reception of certain Discina-like fossils of the Swedish Primordial, and describes under it two species, *A. coriacea* and *A. granulata.* Upon the characters of these two forms I beg leave to offer a few remarks.

Acrothele differs from *Discina*, according to M. Linnarsson's description, in the absence of the longitudinal slit of *Discina*, the perforation of the apex of the ventral valve, and in its interior markings so far as these have been made out. The shell substance is corneous. M. Linnarsson considers the most nearly related genera to be *Obolella* and *Acrotreta*, but at the time he wrote, shells having a phosphatic test had, as he states, been referred to the former genus. M. Linnarsson gives numerous illustrations of the ventral valve of *A. coriacea*, and one of the interior of the dorsal valve which presents some remarkable features. The perforation in the apex of the ventral valve can, he states, usually be made out only with difficulty owing to its extreme minuteness; and in the description of the ventral valve of *A. granulata* no such perforation is shown to exist. In a later illustration of *A. granulata*,[*] however, the apex is represented as perforated, but the perforation is so small that a doubt naturally arises as to whether it truly represents an orifice leading into the interior. Nevertheless M. Linnarsson states that in the internal casts of the ventral valve of *A. coriacea* the presence of an umbonal orifice is always readily recognizable. The interior of this valve, so far as known, shows no trace whatever of muscular scars or imprints.

In that subdivision of the American Primordial known as the Lower Potsdam, and which is considered to lie above the *Paradoxides*-bearing strata of this continent, we have at Troy, N. Y., a species of *Lingulella* (*L. cælata*), very closely resembling in the interior markings of its dorsal valve the dorsal valve of Linnarsson's *A. coriacea*, and in the same formation, both in New York and Canada, another form bearing an equally strong resemblance to his *A. granulata.* This latter American form is the *operculum of a Pteropod.* Its true character was first ascertained by Mr. Billings, who described it, together with the shell to which it belongs, in the Canadian Naturalist for December, 1871, under the name of *Hyolithellus micans.*[†] It is usually circular in form, rarely broad-ovate longitudinally, and has an excentric apex or nucleus. Around this point the surface lines are arranged concentrically. Some of the specimens show also the existence of fine radiating lines in the nucleal region. The outer surface has usually a dullish aspect contrasting strongly with the inner one, which is polished and glistening. On the interior there are ten elongate-ovate muscular impressions having a stellate arrangement. The smaller of these occur on the dorsal side of the operculum. They all terminate in the pit directly beneath the nucleus. The shell and operculum are composed of a corneous substance, which has a finely lamellar structure. None of the specimens of the operculum have a perforation in the nucleus, but its extreme tip occasionally breaks away and gives it such an appearance. In this respect it recalls *Acrothele granulata*, in which the presence of an apical foramen does not appear to be a constant feature. The interior of *A. granulata* has not been observed and the dorsal valve is in doubt. The form is nearly circular, the deviation from this shape being transverse to that of the operculum of *H. micans*. The smallest specimen figured is 4 lines in width, and the largest about 5½ lines.

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[*] "Om faunan i lagren med *Paradoxides ölandicus;*" af G. Linnarsson. (Afdrag ur *Geologiska* Föreningens i Stockholm Förhandlingar 1877, No. 40, Band III, No. 12).

[†] *See* also this Journal for May, 1872.

Associated with *Hyolithellus micans* we have at Troy three species of *Hyolithes*, and in the case of each the operculum has been identified and described. Two of these, *H. Americanus* and *H. impar*,* permit us to observe the interior of this piece. In neither do we find any trace of muscular scars. In this respect they recall the ventral valve of Linnarsson's *A. coriacea*. In all true *Hyolithes* the operculum appears to be invariably divided more or less distinctly into a dorsal and ventral limb. The majority if not all of the species have a calcareous shell, which in some cases is greatly thickened.† In *Hyolithellus*, on the other hand, the test is of extreme tenuity and corneous, and the division of the operculum into dorsal and ventral limbs only very obscurely indicated. Singularly enough, on the second plate accompanying his later paper referred to M. Linnarsson figures side by side an example of the ventral valve of his *A. granulata*, and the apical portion of a new species of *Hyolithes*, showing in its transverse section an outline corresponding almost exactly with that of the adjacent figure of *A. granulata*. Contrary to M. Linnarsson, the *Discina primæva* of Barrande and Verneuil,‡ if not a true *Discina*, appears to me much more probably an operculum than an *Acrothele*, as that genus is at present defined.

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* The latter described by the writer in this Journal for June, 1872.

† M. Barrande, in his description of the Pteropoda of the Bohemian Basin, states that certain of his species of *Hyolithes* having a very thin test appear to make an approach to a corneous composition; but such a composition is not positively stated to characterize any of them (Système Silurienne, &c., iii, 1867, p. 66).

‡ Bull. Soc. Geol. Fr., *t. xvii, p. 532,* pl. VIII, fig. 2.

I do not intend by the foregoing observations to assert that the dorsal valve of M. Linnarsson's *A. coriacea* is the dorsal valve of a *Lingulella*, or that what he sets down as ventral valves of *A. coriacea* and *A. granulata* are both, or either of them, or any of the forms included under them, opercula of *Hyolithes* or Hyolithoid species, or to undertake to decide upon their nature or affinities in one way or another; but only to point out the manifest resemblances between the American and Swedish forms compared, and which appear to me to be sufficiently pronounced to at least suggest the question whether the Swedish species noticed may not, as a whole or in part, be susceptible of a more rigorous determination.

New York, January 22, 1878.

Art. LIII.—*On the "Geodes" of the Keokuk Formation, and the Genus Biopalla, with some Species;* by Samuel J. Wallace, of Keokuk, Iowa.

The large hollow stone balls, set inside with myriads of brilliant crystals, which are found in the upper beds of the Keokuk Formation (Subcarboniferous), are well known for their beauty and as curiosities, under the names of Geodes, Niggerheads, etc. They are very plentiful, of various sizes, from a few lines to over two feet, where that part of the formation is exposed in the Mississippi Valley, and in the lower Drift and the Alluvium derived from it.

This part of the Keokuk Formation, known as the Geode Bed, is exposed in a region about sixty by one hundred miles, with Keokuk, Iowa, as its center, where it is forty feet thick, covered by the St. Louis Formation. A few miles south it descends to the river level, from which it rises and disappears from the highest bluffs fifty miles north and south. About thirty miles east and west it disappears at the bottom of the deepest streams, under the Coal-measures of Illinois, and of Iowa and Missouri. It extends from the southeast corner of that space passing southeast and south beyond St. Louis; and it may extend also southwest through central Missouri. To the east it reappears along the eastern margin of the coal field through Indiana, probably into Kentucky, and as Professor Safford thinks, into central Tennessee. To the west it possibly reappears in the geodes found by Professor Comstock in the Wind River region, Government Survey.

The matrix bed is generally shale, varying sometimes to limestone and to porous rotten stone. The lower layers are limestone but not so pure as the next layer below, which here is the best quarry rock of the Keokuk Formation. Stratification marks often show over and around the geodes similar to those around bodies in mud banks formed by currents. The shale seems to be but the remains of original deposits several times thicker, which have evidently been dissolved out, leaving the insoluble portion to be compressed to the present shale. The limestone portions have not been compressed so much.

The geodes themselves are merely crystalline shells formed from percolating water around the walls of vacant cavities. The outer shell is silica, generally chalcedony, with crystals of various minerals, principally silica and calcite, pointed inward in great variety and beauty. The external forms are sharply marked as casts, as the shale is readily removed by frost and water, leaving the silica walls perfect and permanent. The shell walls are of all degrees of thickness, from perfectly solid to thin crusts, with sometimes no deposits at all in the still perfect cavity. Sometimes they are full and perfect in form, and in others they are more or less crushed, often quite flat. In those that are crushed the outer shell shows the fracture still sharp, but all held together by the inner portion deposited since the crushing. This crushing has been caused by the compression of the strata before referred to. But it shows that the dissolving out of part of the original deposit was after the partial deposit of the geode shell.

The origin of these cavities in the sedimentary deposit has been generally regarded as a mystery. That the shells were formed by crystallizing from water in the cavities is evident from their structure, which often shows fine bands of varied colors, etc., like agates; and by the fact that the cavities are sometimes still entirely vacant.

The Indiana Geological Report, 1873, page 278, gives the idea that they owe their origin to animal remains. This is plausible from the fact that they characterize a single formation and part of a formation, even, in the usual manner of fossils, (though I find cavities, in a certain bed of the St. Louis above, which I suspect had the same origin). It is rendered more evident by the peculiar family likeness through a great variety of sizes and forms; and by the lack of any other cause for them in such remarkable numbers, shapes and sizes.

An extended study of thousands of specimens and exposures, by the writer, confirms this by the recognition of peculiarities of growth and nature. It seems that the cavities were formed by *the rotting out of sponges* which had become covered by deposits. They were somewhat similar to the present horn sponges of trade, but without stems or apparent means of attachment. They may have often been movable, sometimes drifting along on the soft bottom, and often became covered in the soft deposits. They also often grew in fixed positions, as they are found crowded and fitting together in beds, with angular forms. It is evident that only those which became covered by deposits would leave any remains in the strata.

There is a great variety of forms and markings, but they are mainly of a few general and related types. The principal type is that of a massive peculiar cushion-like figure, with indrawn centers and connecting wrinkles, the intermediate spaces swelling lobe-like, more or less deeply marked, so as to resemble the features formed by holding stitches in stuffed cushions and upholstery. The indrawn centers probably represent the mouths of circulation-canals. There are some which do not show these peculiar markings, especially among the smaller ones, and in those which are crowded together in beds. But they were no doubt of the same nature. The several sides are similar, but the top is usually distinct from the lateral sides. They are frequently nearly round, sometimes higher than wide, but usually have three different diameters, the shortest of which is vertical, and so form a flattened oval.

There are indications that the structure was fibrous; the fibers mainly running conformable to the surface and to the wrinkled lines, toward some of the largest centers, at which they passed inward. Some have cup-like cavities of gash or almygdaloid form, some open, and others more or less closed. They have their longer diameters, one-third to one inch, conformable in direction to the supposed fibers and wrinkles.

The writer has a fine specimen which has grown over and around a projecting rock in an interesting way. Mr. L. A. Cox has one, five inches in both diameters, which has grown in a large crinoid stem. It has split the column in five parts, bending them apart to fit its form, into which they are imbedded up the sides. This is well preserved and curious.

Many fine as well as large specimens exist in collections here, and enough for many car-loads have been shipped away. The largest, showing the outside markings finely, is owned by R. F. Bower. It is twenty-six inches across. A larger one, twenty-eight inches, is in the Keokuk Library Collection.

The most promising field to look for details of structure is in the chert forming a large part of the Keokùk formation, which contains remains of various forms of life of those seas, often in better condition, as to details, than are elsewhere found.

Sponges which show internal structure are known from various formations. This generally arises from their being mineral sponges, or from their having been silicified before the original structure disappeared. In the Keokuk formation I know of nothing, outside of the chert, where a non-mineral body was silicified so as to show internal structure. All the non-mineral substance, and many parts formed of lime, have first entirely disappeared, so as to leave vacant cavities. Some of these are still vacant. But most have been afterward filled by crystals from water containing bicarbonate of lime and silica, which show no internal structure of the original body. This includes not only these grades, but a large proportion of the crinoids, shells and corals, that originally contained lime; so that this is not so strange for the non-mineral sponges as for those.

The following is the principal distinct type:

BIOPALLA (new genus).

Body, subglobular, varying in growth; size, usually a few inches, varying from one line to over two feet; no foot-stalk or apparent means of attachment; top, bottom, and lateral faces often distinct, but similar.

External markings, in large specimens, of two kinds of peculiar indrawn features: 1st. Indrawn centers, at unequal intervals over the surface (often one or more pit marks each), perhaps mouths of circulation-canals; 2d. Indrawn wrinkles, connecting, usually radiating from, one to another of the largest centers, in an irregular net work; and with smaller indrawn centers along the furrows. Surface, swelling lobe-like between the centers and furrows into a more or less deeply marked peculiar cushion-like figure.

Named from the Greek, *βίος*, life, and *παλλα*, a ball.

There is uncertainty as to the distinction of species, but I venture to name the following:

Biopalla Keokuk.—Typical species; size medium to large; form nearly rounded to flattened-ovoid; markings those of the genus, distinct, medium to numerous, varied in size and mode of distribution on same and different specimens.

From Keokuk, Iowa; Hamilton, Ill., and surrounding region.

Biopalla grandis.—Secondary type; size medium to largest; form ovoid horizontally; sides similar. Markings, those of the genus, very distinct, few to medium, symmetrical and regular. Interspaces large, swelling lobe-like. A few largest indrawn centers have peculiar radiating furrow marks, melon-like.

From Keokuk, Iowa; Clark County, Mo., and surrounding region.

Biopalla Wortheni.—Size medium to large; form varied; vertical and lateral faces different. Markings on top more or less sharp and crowded; on lateral sides, less numerous, and elongated vertically; on bottom not so sharp as on top; otherwise more or less varied, as in *B. Keokuk.*

From Hamilton, Ill.; Drift; and other places.

Biopalla Woodmani.—A peculiar form from the Drift, Keokuk, Iowa, supposed to be from the northward. Found as fragments; flattened by crushing before the crystalline deposit was completed. Interior often with brown, coarse, gritty clay from matrix, cohering by slight crystalline substance. Original form not known. Size medium to large. Markings, indrawn centers not distinctly defined, but indrawn lines distinct, forming a more or less intricate net work, with lines generally directed to the sides on top and bottom. The interspaces generally projecting prominently, as if a soft substance was forced through the meshes of a net, with some resemblance to a brain-like surface.

Biopalla Hæckeli.—Size medium to ——. Form sometimes flattened. Markings often distinct. Surface with more or less open gash-shaped cup-like cells, differing in size with the body, one-third to one inch in longest diameter, in directions conformable to those of the furrows.

A peculiar form. Keokuk, Iowa; Canton, Mo.; Drift, etc.

Biopalla Hyatti.—Size small to medium; form varied, generally elongated cylindrically. Markings distinct; on lateral or cylinder sides, centers usually on furrows that run crosswise or around the cylinder, often deep. Ends sometimes rounded; often flattened by contact.

From Keokuk, Iowa; Hamilton, Ill.; Drift, etc.

Biopalla Alicei.—Size small to medium. Form more or less flattened, lateral edges thin and centers more or less projecting. Markings generally not deep; often pit-like marks; sometimes an indrawn furrow runs diagonally from bottom to top or around, with centers along it. Often beautifully translucent, with peculiar markings.

From Drift, Keokuk, Iowa, etc.

Biopalla palmata.—Size medium to ——. Form flattened. Markings not deep on top and bottom, but elongated toward the edges; the edge deeply serrated by projecting interspaces, and deeply indrawn vertical furrows.

A rare and peculiar form. Keokuk, Iowa, Drift.

ART. LIV.—*The Coralline, or Niagara Limestone of the Appalachian System as represented at Nearpass's Cliff, Montague, New Jersey;* by Dr. S. T. BARRETT, Port Jervis, N. Y.

AT Mr. Wm. Nearpass's quarry, three miles southwest of Port Jervis, is a nearly perpendicular cliff over two hundred feet in height, which shows at top one hundred feet of exposed strata. The Stromatopora limestone, Favosite limestone of my former communication,* and the Tentaculite limestone with its two divisions of dark blue and quarry stone occupy the upper twenty feet of this cliff, while below the Tentaculite, and paleontologically connected with it, are nearly horizontal strata, about thirty feet in vertical thickness, apparently referable to the Water Lime division of the Lower Helderberg group. Lying below the Water Lime are fifty feet vertical thickness of exposed rock which contain species characteristic of the Coralline limestone at Schoharie, with a larger proportion of Niagara species than are reported from that locality, a few Clinton types and some perhaps new or peculiar species.

These species as far as identified are as follows:

Coralline limestone species: *Cyathophyllum inequale*=*Columnaria inæqualis*, *Strophodonta* —— =*Leptæna* —— of Plate 74, figs. 3*a* and 3*b*, Pal. N. Y., vol. ii, *Rhynchonella lamellata*=*Atrypa lamellata*, *Meristella nucleolata*=(*Atrypa*) *nucleolata*, *Calymene camerata;* all of which were identified by Mr. Whitfield; *Stromatopora constellata*, *Tellinomya* (?) *aequilatera* and *Avicula securiformis*, identified by myself.

Niagara species: *Halysites agglomeratus*, *Favosites pyriformis*, *Cladopora seriata*, *Cyatho Shumardi*, *Rhynchonella pisa;* identified by Mr. Whitfield; *Halysites catenulatus*, *Syringopora multicaulis*, *Favosites venustus*, *F. parasiticus*, *Stromatopora concentrica*, *Trematopora tuberculosa*, *Aulopora precius*, *Spirorbis inornatus*, *Pholodops ovalis* and *Ambonychia acutirostra*, identified by myself.

Clinton species: *Caninia bilateralis* by Mr. Whitfield, *Tentaculites minutus* and *Beyrichia lata* by myself.

A very beautiful *Proetus* of about the size and general outline, as far as can be conjectured from the fragments in my possession, of the *P. Stokesii* (?), Pal. N. Y., vol. ii, Pl. 67, occurs, very rarely, throughout this lower fifty feet. The pygidium is subsemicircular, narrowly rounded behind, margined. Lobes subequal, mesial lobe elevated, obtuse posteriorly, number of segments thirteen or fourteen, continued backward to the end. Surface of the cheeks and margin of the pygidium vermiculately striate, otherwise, as far as known, granulate. Inferior marginal portions of the pygidium and cephalic shield incrassated with imbricating lamellæ, appearing much as represented in fig. 4*a*, Pl. 66, vol. i, N. Y. Pal. This may be the same species doubtfully referred by Professor Hall to the *Proetus Stokesii* of Murchison, but differs very much from the figures and description there given. I have named it, provisionally, *Proetus pachydermatus.*

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* This Journal, vol. xiii, pp. 385 and 386. The Stromatopora Limestone is best seen at Mr. Sandford Nearpass's Quarry, ¼ mile northeast of Nearpass's Cliff.

The *Strophodonta*[*] (*Leptæna*) —— of Pl. 74, figs. 3*a* and 3*b*, Pal. N. Y., vol. ii, is very abundant in the lower beds of this Coralline or Niagara limestone. Its ventral valve has the surface characters represented enlarged in fig. 3*b*,[†] its dorsal valve has the flat radiate striæ and the concentric, crowded, thread-like striæ represented in fig. 6*d* of the same plate. Both valves have a denticulated hinge line, the cast of the interior of the ventral valve resembles fig. 4*a*, the cast of the interior of the dorsal valve is near figs. 6*a* and 6*b* of the same plate. The impression of the cast of the interior of the dorsal valve shows widely divergent socket-ridges,[‡] with three subparallel ridges in the bottom of the shell, the mesial longest and extending two-thirds the length of the valve toward the front. Old shells have about the size and form of fig. 4*a*. Shell flat, undulated, sometimes slightly resupinate, interior punctate. There are layers, I think from the lower unexposed part of the cliff, represented by bowlders at its foot, in which this shell is very closely packed. It, the *Rhynchonella lamellata*, *Proetus pachydermatus* and *Meristella nucleolata* originate in rocks of probable Clinton age and are continued upward into the Coralline. The *Cyathophyllum inequale* and most of those enumerated under the head of Niagara species are restricted to the upper half of this lower fifty feet. The total thickness of the Coralline limestone at this place cannot now be given, because its lower boundary is buried beneath the talus which covers the lower half of the cliff. The number of species belonging to the Coralline and Niagara limestones can be no doubt increased by further examinations, while the number of new species is probably no greater than has been found at other places where the Niagara has been examined. The, at least apparent, absence of many of the Coralline limestone species and the increase in the number of accepted Niagara species at this place agrees with the theory that the Coralline limestone is the thinned eastern edge of the Niagara deposits, which as it is traced southwestwardly along the Appalachians shows a fading out of Coralline and an increase of Niagara types.

Of the few Coralline limestone species, only the *Cyathophyllum inequale* and the *Strophodonta* (*Nearpassii?*) are abundant here, the other species being represented by a very few depauperate specimens only. The spherical and sub-pyramidal forms of Favosites are very abundant here as well as at Schoharie, and seem referable to species described by Professor Hall in the N. Y. Pal. for the Niagara group. The mural pores show plainly in thin vertical sections, or, better yet, in sections cut obliquely transverse to the axis of growth.

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[*] I have labelled it provisionally *S. Nearpassii.*

[†] The postero-lateral radiate striæ are more arcuate.

[‡] Cardinal processes not apparent.

Art. LV.—*Descriptions of new Genera and Species of Isopoda, from New England and Adjacent Regions;* by Oscar Harger. *Brief Contributions to Zoology from the Museum of Yale College,* No. XXXVIII.

The genera and species described in the present paper are, except the first, marine and were, mostly, collected by the United States Fish Commission, along the New England coast. More complete descriptions with figures of all the new, and most of the old species, are nearly ready for publication in the Report of the Commissioner. As it seems desirable, however, to give a wider publication to the genera and species believed to be new, the following diagnoses are here inserted.

Actoniscus, gen. nov.[*]

Eyes small. Antennæ geniculate at the third and fifth segments; flagellum four-jointed. Terminal segment of maxillipeds lamelliform. Legs all alike. Pleon of six distinct segments. Basal segments of uropoda dilated and simulating the coxæ of the preceding segments; rami both styliform.

This genus belongs to the *Oniscidæ* and is near *Actæcia* Dana, MSS. (U. S. Expl. Exped., Crust., part II, p. 736, pl. 48, fig. 6), regarded as the young of *Scyphax*, but considered by Kinahan as the representative of a distinct family of the Oniscoidea.

A. ellipticus, n. sp. Body oval. Head with a prominent angular median lobe, and broadly rounded, divergent lateral lobes. Eyes oval, longitudinal, prominent, black. Antennulæ rudimentary. Antennæ nine-jointed; first segment short; second strongly clavate; third smaller, clavate; fourth flattened-cylindrical; fifth longest, slender, bent at the base; flagellum shorter than the fifth segment, composed of four subequal segments, tipped with setæ. Terminal segment of maxillipeds elongate triangular, ciliated and slightly lobed near the tip. First thoracic segment excavated in front for the head, shorter above than the following segments except the last, which is shortest. Legs small, scarcely spiny. Pleon continuing the regular oval outline of the thorax, apparently with four pairs of lamellar coxæ, the last pair are, however, the enlarged basal segments of the uropoda and are notched on their inner margins for the short outer rami, while the more slender inner rami are borne lower down on the under surface. The rami scarcely project beyond the general outline.

This species has been collected by Professor A. E. Verrill, at Savin Rock, near New Haven, and also at Stony Creek, in company with *Philoscia vittata* Say.

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[*] From ἀκτή, the beach, and *Oniscus*.

Chiridotea,* gen. nov.

First three pairs of legs terminated by prehensile hands, in each of which the carpus is short and triangular, the propodus is robust and the dactylus capable of complete flexion on the propodus. Antennæ with an articulated flagellum. Head dilated laterally. Operculum vaulted, with two apical plates.

This genus is founded on *Ch. cœca* (*Idotea cœca* Say), which occurs on this coast from Florida to Halifax, Nova Scotia. It includes *Ch. Tuftsii* (*Idotea Tuftsii* Stimpson), of the New England coast from Long Island Sound to the Bay of Fundy, and, as constituted above, would also include *Ch. entomon* (*Idotea entomon* Bosc.), from the Baltic and other European localities, and *Ch. Sabini* (*Idothea Sabini* Kröyer), from the Arctic. The above mentioned species ought certainly to be separated from *Idotea tricuspidata* Desm., which may properly be regarded as the type of the genus *Idotea* Fabr.

Synidotea,† gen. nov.

Antennæ with an articulated flagellum. Epimeral sutures not evident above. Pleon apparently composed of two segments, united above but separated at the sides by short incisions. Operculum with a single apical plate. Palpus of maxillipeds three-jointed.

This genus is founded on *S. nodulosa* (*Idothea nodulosa* Kröyer), who appears to have been misled, in his unnatural description of the epimera, by the marginal thickening of the segments. He describes the epimera as evident even on the first segment.

Astacilla Americana, sp. nov.

Body nearly uniform in size throughout in the female, with the fourth thoracic segment narrow in the male, tuberculated. Head united with the first thoracic segment, and, together with it, twice the length of the next two segments; excavated in front, with the sides extending beyond the middle of first antennular segment, tuberculated above, crossed between and behind the eyes by two transverse grooves, while a third groove indicates the suture of the first thoracic segment. Eyes lateral, round-ovate, broadest in front. Antennulæ four-jointed, slightly surpassing the second segment of the antennæ in the female, nearly attaining the middle of the third in the male; basal segment swollen, nearly as long as the next two which are much more slender, last or flagellar segment shorter than the peduncle in the female, longer than the peduncle in the male. Antennæ about three-fourths as long as the body, fourth segment longest, then the fifth and third; first two segments short; flagellum three-jointed, short. First thoracic

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*** From χέιρ, a hand, and Idotea. † From σύν, with or together, and Idotea.**

segment embracing the head, separated from it by sutures at the sides, but united in the dorsal region. Fourth segment longer than the other six segments together in the female, still more elongated in the male, in which sex it is longer than the three following segments with the pleon, while in the female it is only four-fifths as long; irregularly but not coarsely tuberculated, especially in the dorsal region. Last three segments with their epimeral regions produced into salient angular tubercles. Pleon elongate-ovate, tuberculated, a little longer than the last three thoracic segments, with three transverse grooves in the proximal region, the second continued at the sides, but showing no distinct suture. Immediately behind this is a prominent tooth on each side, directed outward and backward. Tip of pleon not spiniform but only slightly attenuated and obtuse. Opercular plates more than nine-tenths as long as the inferior surface of the pleon.

Length of female 10mm., male 11mm.; diameter of fourth thoracic segment, female 1·2mm., male 0·52mm. Of the two adult specimens obtained, fortunately a pair, the male, though much the more slender, actually exceeds the female in length. This relation of size in the sexes is unusual in the genus, the females being generally considerably larger than the males, but more specimens are necessary to prove the constancy of this proportion.

The specimens of this species were found adhering to *Primnoa*, from St. George's Bank.

Astacilla Fleming, is synonymous with *Leacia* (*Leachia*) Johnston, which is preoccupied.

Eurycope robusta, sp. nov.

Body oval, smooth and polished, breadth nearly equal to half the length. Head longer than the first thoracic segment, produced medially into a short rostrum about half as long as the basal antennular segments. Antennulæ attaining the middle of the fourth antennal segment; basal segment subquadrate, bearing the second, much smaller, segment beyond the middle of its superior surface; third segment slender; flagellum about twice as long as the peduncle, multiarticulate. Antennæ thrice the length of the body at least in the female; first three segments short; fourth and fifth slender, subequal and together as long as the body in the female; flagellum long, slender and multiarticulate. External lamella of maxillipeds subrhombic, with the inner angle much rounded, the outer prominent but not acute. First four thoracic segments short; fourth widest, fifth and sixth suddenly twice as long; seventh much the longest of all. First pair of legs shorter than the body; carpus exceeding the propodus; second pair longer than the body; third and fourth increasing slightly in length; carpus and propodus subequal in all, armed, in the second pair only, with spines. Swimming legs (last three pairs) robust, carpus subcircular, dactylus usually about half as long as the propodus. Pleon broader than long. Uropoda short, rami cylindrical, spiny at the tip; the outer more slender but not shorter than the inner. Length of body 4·5mm. Carpus of first pair 1mm.; propodus 0·6mm.; of second pair, carpus 1·5mm., propodus 1·6mm.; of fourth pair, carpus 1·5mm., propodus 1·7mm. Color, in alcohol, pale honey-yellow.

This species was dredged in 220 fathoms, in the Gulf of St. Lawrence, by Mr. J. F. Whiteaves.

Ægathoa loliginea, sp. nov.

Body elongate oval, not suddenly narrower at the base of the pleon, which is slightly dilated at the last segment. Head subequally, but not deeply, lobed behind, the lateral lobes being formed by the large, semi-hexagonal, coarsely reticulated eyes, which cover half the upper surface of the head. Antennulæ as long as the head, eight-jointed, separated at their bases, tapering to the tip; antennæ more slender, ten-jointed, surpassing the antennulæ by the last two segments, like the antennulæ without evident division into peduncular and flagellar segments. First thoracic segment shorter than the head and but little broader, not embracing it at the sides, longer than the following segments, which increase in width to the fifth; seventh shortest. Epimera short and obtuse, not surpassing the rounded posterior angles of the segments. Legs nearly alike throughout, first pair a little more robust, last pair slightly the longest, all with strongly curved dactyli. Pleon longer than the thorax, tapering to the fifth segment. First pair of pleopoda with the basal segment large, nearly square; last pair, or uropoda, surpassing the telson; basal segment triangular with the inner angle acute but scarcely produced; rami flat, the outer with slightly divergent sides, obliquely rounded at the end; the inner broader, triangular, with the outer side longest; cilia very short almost rudimentary. Length 13mm., breadth 3·6mm. Color in alcohol yellowish with minute black specks, most abundant on the pleon. Eyes black.

The only specimen in the collection was obtained by Mr. S. F. Clark, at Savin Rock, near New Haven, from the mouth of a squid (*Loligo Pealii*), whence the specific name.

Ptilanthura, gen. nov.*

Antennulæ with the flagellum remarkably developed, multiarticulate, second and following segments provided with an incomplete, dense whorl of fine slender hairs. This whorl is interrupted in each segment upon its internal or anterior side,

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* From *πτιλόν* a plume, and Anthura.

which, however, in the ordinary reflexed position becomes the external side. Eyes distinct, visible both above and below. Pleon imperfectly segmented, elongate. Maxillipeds two-jointed.

P. tenuis, sp. nov. Body smooth, slender, flattened above, broadest at the base of the pleon. Head broader but shorter than the first thoracic segment, narrowed to a point in front and less acutely behind. Eyes prominent, black, within the margin of the head. Antennulæ, when reflexed, attaining the third thoracic segment; first segment large but not longer than the second; third shorter than the second, followed by a short first flagellar segment, second and following segments about twenty in number, obconic, fitting into each other, flattened and naked on one side, which is the outer and somewhat inferior side in the reflexed organ, densely elongate-ciliate distally, except on the flattened side; cilia attaining about the fifth following segment. Antennæ hardly surpassing the peduncle of the antennulæ, eight-jointed. Maxillipeds with a quadrate basal segment, emarginate externally for the subtriangular external lamella, and bearing a single scarcely smaller terminal segment, truncate and ciliate at the tip. Thoracic segments slender, margined, the seventh but little over half as long as the others. First pair of legs moderately enlarged, segments well separated, dactylus strong, shorter than the inner margin of the propodus; remaining pairs of legs slender. Pleon about as long as the last three thoracic segments, first five segments consolidated along the median line, each rising into a low broad tubercle on each side of the median line; last segment as long as the preceding five; telson elongate-ovate obtusely pointed. Uropoda equaling the telson. Length 11mm., breadth 0·9mm., color in life brownish and somewhat mottled above, lighter below.

This species has been found on the New England coast from Noank Harbor, Conn., to Casco Bay, Maine.

Paratanais algicola, sp. nov.

Tanais filum Harger, Rep. U. S. Com. Fish and Fisheries, part 1, p. 573. 1874, *non* Stimpson.

Eyes conspicuous, black, plainly articulated, larger in the males. Antennulæ in the females three-jointed, tapering, setose at the tip, first segment as long as the last two which are subequal; elongated and eleven-jointed in the male, the first segment long, curved upward near the base, last eight segments with olfactory setæ. Antennæ short, five-jointed, deflected, fourth segment longest. First pair of legs robust, hand short and stout in the female, digital process scarcely toothed, bearing three setæ near its inner margin; hand in males strongly chelate, digital process elongated, curved, two-toothed; dactylus curved, slender, with about seven setiform spines on its inner margin; carpus in the males long and stout.

Second pair of legs elongated, basis flattened and curved, dactylus slender but shorter than the propodus. Bases of last three pairs of legs swollen. Uropoda bearing setæ at the tips of the segments, biramous; outer ramus short, scarcely if at all surpassing the basal segment of the inner ramus which is six-jointed and tapering. Length 2·2mm., breadth 0.33mm. Color nearly white.

This species is rather abundant among eelgrass and algæ at Noank and Woods-Holl, and probably other localities on the southern shore of New England. I formerly considered it as identical with *Tanais filum* Stimpson and supposed its range to extend as far as the Bay of Fundy. I now regard that as an error, as it is probable that *T. filum* is a true *Tanais* with simple uropoda, though I have as yet seen no specimens from the Bay of Fundy, nor any fully answering to Stimpson's description.

Paratanais limicola, sp. nov.

This species considerably resembles the preceding, but may be distinguished from it by the following characters: The eyes are small and rather inconspicuous, at least in the females, being only about half the vertical diameter of the antennulæ. The antennulæ have the second segment short, about half as long as the third. The dactylus of the second pair of legs, with its slender, acicular, terminal spine is longer than the propodus. The pleon is not dilated at the sides. The uropoda have the outer ramus two-jointed, slender, and surpassing the basal segment of the inner ramus which is five-jointed, with the basal segment long and imperfectly divided. Length 2·5mm.

This species was obtained on a soft muddy bottom in forty-eight fathoms, Massachusetts Bay, off Salem, in the summer of 1877, by the United States Fish Commission.

Paratanais cæca, sp. nov.

Body slender, elongated and loosely articulated. Head narrow in front, not broader than the bases of the antennulæ. Eyes wanting. Antennulæ four-jointed; first segment forming less than half its length; second segment longer than the third; last segment about as long as the second, slender, tapering and tipped with setæ. Antennæ attaining the tip of the third antennular segment. First pair of legs slender as compared with those of the preceding species, attaining the tip of the antennæ, basal segment subquadrate, hand or propodus less robust than the carpus; digital process of propodus serrated; dactylus short. Second (first free) thoracic segment two-thirds as long as the third, which is equal to the fourth and fifth; sixth and seventh progressively shorter. Second pair of legs scarcely more slender than the following pairs, basal segment not curving around the basal segments of the first pair. Pleon six-jointed; uropoda short, biramous, each ramus two-jointed, the outer more slender than the inner, half its length and bearing a long bristle at the tip. Length 2·5mm.

This species was taken along with *P. limicola* and unfortunately only a single specimen is as yet known.

Yale College, April, 1878.

ART. LVI.—*Ammonio-argentic Iodide*; by M. CAREY LEA.

WHEN silver iodide is exposed to ammonia gas it absorbs 3·6 per cent, and forms according to Rammelsberg a compound in which an atom of ammonia is united to two of AgI. Liquid ammonia instantly whitens AgI, every trace of the strong lemon-yellow color disappears. The behavior of the ammonia iodide under the influence of light differs singularly from that of the plain iodide, and will be here described.

The affinity of AgI for ammonia is very slight. If the white compound be thrown upon a filter and washed with water, the ammonia washes quickly out, the yellow color reappearing. If simply exposed to the air, the yellow color returns while the powder is yet moist, so that the ammonia is held back with less energy than the water. So long, however, as the ammonia is present, the properties of the iodide are entirely altered.

AgI precipitated with excess of KI does not darken by exposure to light even continued for months. But the same iodide exposed under liquid ammonia rapidly darkens to an intense violet-black, precisely similar to that of AgCl exposed to light, and not at all resembling the greenish-black of AgI exposed in presence of excess of silver nitrate. (This difference no doubt depends upon the yellow of the unchanged AgI mixing with the bluish-black of the changed, whereas in the case of the ammonia iodide the yellow color has been first destroyed.)

When the exposure is continued for some time, the intense violet-black color gradually lightens again, and finally quite disappears, the iodide recovers its original yellow color with perhaps a little more of a grayish shade. This is a new reaction and differs entirely from anything that has been hitherto observed. It has been long known that darkened AgI washed over with solution of KI and exposed to light, bleached. This last reaction is intelligible enough for KI in solution exposed to light, decomposes, and in presence of AgI darkened by light gives up iodine to the AgI and so bleaches it. The above experiment is quite different. The darkened substance may be washed well with water (during which operation it passes from violet-black to dark-brown), and may then be exposed to light either under liquid ammonia or under pure water, in either case the bleaching takes place, though in the latter case more slowly.

If the experiment be performed in a test-tube, the bleaching under ammonia requires several hours, under water from one to three days. But if the iodide be formed upon paper, and this paper be exposed to light, washing it constantly with liquid ammonia, the darkening followed by the bleaching requires little more than a minute. In this case, however, the bleaching is not so complete, perhaps because of the influence of the organic matter present. The bleaching appears to depend upon the escape of ammonia, for if the darkened ammonia iodide is covered with strong liquid ammonia and the test-tube well corked, the bleaching does not take place.

It became a matter of interest to know whether the darkening under ammonia was accompanied by any decomposition; whether the ammonia took up iodine from the silver salt under the action of light. For this purpose AgI was precipitated with excess of KI and subjected to a long and thorough washing; it was then exposed for several days to light under strong liquid ammonia. As AgI is not wholly insoluble in ammonia, the mother-water was first evaporated to dryness at a heat but little over ordinary temperatures. The traces of residue were washed with water, and this water gave distinct indications of iodine. The iodine present is in so small quantity that it may easily be overlooked, but it is certainly there. The washing given to the AgI was so thorough that it seemed impossible to admit that traces of KI remained attached to the AgI, but in order to leave no room for doubt, the experiment was repeated, using an excess of silver nitrate in making the precipitation, followed by thorough washing. Iodine was still found in combination with ammonia, and under these conditions there could be no doubt that AgI had been decomposed.

When AgI is blackened under ammonia in a test-tube, and the uncorked test-tube is set aside in the dark for a day or two, the AgI assumes a singular pinkish shade. It thus appears that AgI under the influence of ammonia and of light gives indications of most of the colors of the spectrum. Starting with white it passes under the influence of light to violet, and thence nearly to black: this violet-black substance washed with water passes to brown. The brown substance covered with ammonia and left to itself in an open test-tube becomes pinkish in the dark, yellow in sunlight. These curious relations to color which we see in the silver haloids, from time to time exhibiting themselves in new ways, seem to give hope of the eventual discovery of some complete method of heliochromy.

Philadelphia, March 25, 1878.

ART. LVII.—*Description of a Fossil Passerine Bird from the Insectbearing Shales of Colorado;* by J. A. ALLEN.*

THE species of fossil bird described in this paper is based on some beautifully preserved remains from the insect-bearing shales of Florissant, Colorado. They consist of the greater part of a skeleton, embracing all of the bones of the anterior and posterior extremities, excepting the femora. Unfortunately, the bill and the anterior portion of the head are wanting, but the outlines of the remainder of the head and of the neck are distinctly traceable. The bones are all *in situ*, and indicate beyond question a high ornithic type, probably referable to the Oscine division of the *Passeres*. The specimen bears also remarkably distinct impressions of the wings and tail, indicating not only the general form of these parts, but even the shafts and barbs of the feathers.

In size and in general proportions, the present species differs little from the Scarlet Tanager (*Pyranga rubra*) or the Cedar-bird (*Ampelis cedrorum*). The bones of the wings, as well as the wings themselves, indicate a similar alar development, but the tarsi and feet are rather smaller and weaker; and hence in this point the agreement is better with the short-legged Pewees (genus *Contopus*). These features indicate arboreal habits and well-developed powers of flight. The absence of the bill renders it impossible to assign the species to any particular family, but the fossil on the whole gives the impression of Fringilline affinities.

Palæospiza bella, gen. et sp. nov.

Wings rather long, pointed. Tail (apparently)† about two-thirds the length of the wing, rounded or graduated, the outer feathers (as preserved) being much shorter than the inner. One side shows distinctly six rectrices. Tarsus short, its length a little less than that of the middle toe. Lateral toes subequal, scarcely shorter than the middle one. Hind toe about two-thirds as long as the middle toe. Feet and toes strictly those of a perching bird, and the proportionate length of the bones of the fore and hind limbs is the same as in ordinary arboreal *Passeres*, especially as represented by the *Tanagridæ*.

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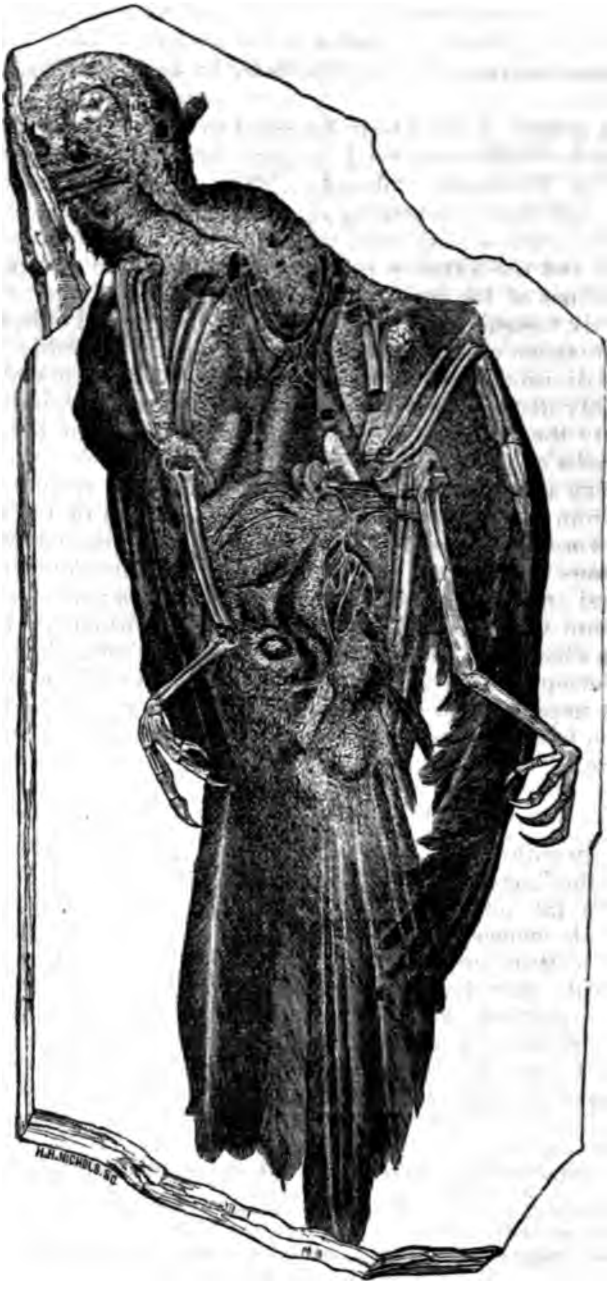
* From the Bulletin of the Geological and Geographical Survey of the Territories, vol. iv, No. 2, page 443, April, 1878.

† The character of the tail is given with reservation, since it is not quite certain that the whole of the tail, or that the exact form of the terminal portion, is shown, especially as the preserved impression is somewhat unsymmetrical.



1.

One of the specimens affords the following measurements:

	Inches.		Inches.
Humerus, length..........	0·80	Middle toe and claw......	0·65
Forearm, length....... .	0·95	Claw alone...............	0·20
Manus, length............	1·02	Hind toe and claw........	0·37
Coracoid, length..........	0·72	Claw alone................	0·15
Clavicle, length...........	0·63	Wing	3·60
Tibia, length..............	1·00	Tail (approximate)........	2·70
Tarsus, length............	0·60	Total length (approximate)	6·85

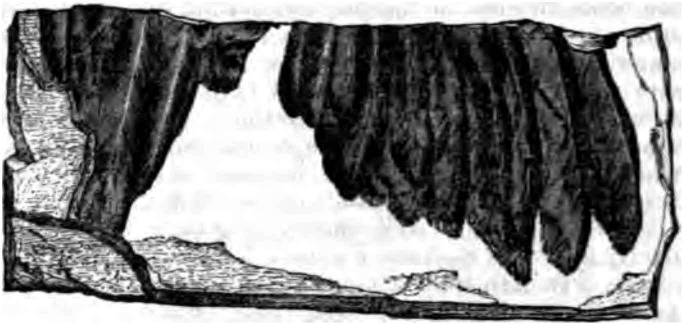
The bones still rest in the original matrix, and, being somewhat crushed and flattened, do not admit of detailed description and comparison with other types. The furculum is well preserved, and the limb-bones are all in place in their natural relation. The sternum is unrecognizable. The position of the cervical series of vertebræ and the general outline of the skull can be traced; but no structural characters of the head can be distinguished, except the proximal portion of the mandible. The long bones all present a well-marked longitudinal groove, due evidently to compression and fracture. This groove is distinctly traceable, even in such slender bones as tibiæ, tarsi, and clavicles. In point of size, while the furculum and the bones of the wing have all about the same length as the corresponding parts in *Ampelis cedrorum*, they apparently are considerably stouter. Their greater breadth may, however, be due simply to flattening from pressure. The tibiæ and tarsi are a little shorter than in the species last named, but the difference is only slight.

The most remarkable feature of the specimen is the definiteness of the feather impressions. Both the shafts and the barbs are shown with great distinctness in the rectrices, and the tips of the primaries of one wing are also sharply defined, overlying the edge of the partly expanded tail. The tip of the opposite wing can also be seen beneath the tail. The feet are so beautifully preserved that even the claws are perfectly distinct. (Fig. 1.)

Another specimen from the same locality, and probably representing the same species, consists of the tip of the tail and about the apical third of a half-expanded wing. (Fig. 2.) In this example the tail is also pointed and graduated. About seven of the outer primaries of the wing are shown with great distinctness, and two others can be easily made out. The third primary is the longest; the second is slightly shorter; the first and fourth are about equal. There are also in the collection three detached contour feathers of small size, but whether pertaining to the same species as the other specimens cannot, of course, be determined.

The larger specimen, first described, is divided into an upper and a lower half, the greater part, however, adhering to the lower slab. The bones adhere about equally to the two faces. The drawing is made from the lower slab, with some of the details filled in from the upper one. The feather impressions are about equally distinct on both, and where in either case the bones are absent, exact molds of them remain, so that the structure can be seen and measurements taken almost equally well from either slab, except that nothing anterior to the breast is shown on the upper slab.

2.



The species here described is of special interest as being the first fossil Passerine bird discovered in North America, although birds of this group have been known for many years from the Tertiary deposits of Europe. The highest extinct ornithic type hitherto known from America is a Picarian bird (*Uintornis lucaris*) related to the Woodpeckers, described by Professor O. C. Marsh in 1872, from the Lower Tertiary of Wyoming Territory. Probably the insect-bearing shales of Colorado will afford, on further exploration, other types of the higher groups of birds.

For the opportunity of describing these interesting specimens I am indebted to Mr. S. H. Scudder, who obtained them during his last season's (1877) explorations of the Florissant insect-beds. The specimens are now the property of the Boston Society of Natural History. My thanks are due to Mr. J. H. Blake for the great care with which he has executed the drawings.

In conclusion, I may add that in 1871 I obtained a few distinct impressions of feathers from beds of the same age and from near the same locality. The first fossil feather, to my knowledge, discovered in North America, was obtained by Dr. F. V. Hayden in 1869, from the fresh-water Tertiary deposits of Green River, Wyoming Territory. This was described by Professor O. C. Marsh in 1870,* who refers to it as "the distal portion of a large feather, with the shaft and vane in excellent preservation."

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* This Journ., II, vol. xi, p. 272, 1870.

ART. LVIII.—*Experiment for Illustrating the Terrestrial Electrical Currents;* by Professor WM. LEROY BROUN.

THE following experiment enables a lecturer to exhibit to a large audience, in a very simple way, the action of the currents of electricity that pass around the earth. The experiment was suggested on reading an article by Professor J. W. Mallet, in the Philosophical Magazine for November, 1877.

A rectangular frame was made of light poplar wood, of section three by two centimeters, whose sides were in length a fraction over a meter, and in breadth three-fourths of a meter. About the perimeter of this rectangular frame were wrapped twenty coils of insulated copper wire: each extremity of the wire was made to terminate near the center of one of the shorter sides, and passing through the wooden frame was fastened and cut off about three centimeters from the frame. This rectangular frame was then so suspended, in a horizontal position, by wires attached to the frame of an ordinary hydrostatic balance, that the longer sides were at right angles with the beam. By adjusting weights in the pans the index of the balance was brought to the zero point. Two small orifices bored in a block of wood, a centimeter apart, served as mercury cups in which the extremities of the short terminal wires were immersed. Near the bottom and through the walls of these wooden cups were screwed small brass hooks, which served as connections, to which the wires of the battery were attached. The balance was now so placed that the longer sides of the suspended rectangle were at *right angles with the magnetic meridian* or in the *magnetic* east and west line.

When the current from the battery was made to pass around the rectangle from east to west on the northern side, and from west to east on the southern side, by the theory of terrestrial magnetism, the northern side of the rectangle would be attracted and the southern side repelled, and that this was so, the corresponding deflection of the balance rendered plainly visible. When the current was reversed the deflection was in the opposite direction. By breaking and closing the circuit at proper intervals to augment the oscillations, the large frame was readily made to oscillate through an arc of five degrees. When the sides of the rectangle were placed northeast and southwest the current produced no sensible effect. A bichromate of potash battery of sixteen cells with plates of zinc and carbon, twenty-five by six centimeters, was used.

With a rectangle containing a larger number of coils of wire, attached to a very delicate balance, by using a *constant acting* battery, the variation in the magnetism of the earth might thus be advantageously observed.

SCIENTIFIC INTELLIGENCE.

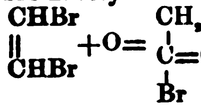
I. CHEMISTRY AND PHYSICS.

1. *On the Specific Heat of Beryllium.*—NILSON and PETTERSSON of the University of Upsala have determined carefully the specific heat of beryllium, with a view to decide upon the position of this metal among the elements. It was prepared by decomposing the fused chloride with sodium in slight excess in an iron cylinder, heated to bright redness. On opening the apparatus after cooling, the upper portion contained a network of brilliant microscopic crystals of metallic beryllium, some of them appearing even to the naked eye distinctly prismatic, often aggregated in dendritic masses of the color and luster of steel. There were found also some fused globules of the metal, of which the largest had a diameter of two millimeters. Beryllium is permanent in the air, does not evolve hydrogen gas with water even on boiling, is easily soluble in dilute acids and in alkali hydrates, with active evolution of hydrogen, is not attacked, even at a red heat, by oxygen or sulphur, and burns readily in a current of chlorine forming the chloride. On analysis, the metal gave of beryllium 87·09 per cent; of beryllium oxide 9·84; of iron 2·08; and of silica 0·99=100·00. Its specific gravity at 9° C., was 1·9101; or, allowing for the impurities present, the specific gravity of pure beryllium is 1·64. The specific heat was determined in Bunsen's ice calorimeter, by Schuller and Wartha's modification. The specific heat of beryllium oxide was found, in a preliminary experiment, to be 0·2471 between 0° and 100° C. As the result of four experiments, the specific heat of pure beryllium was obtained as 0·4107, 0·4144, 0·4001 and 0·4064, giving as a mean the value 0·4079. As with this specific heat the atomic heat would be 3·75 only, if the atomic weight of beryllium be taken at 9·2, but would be 5·63 or normal, if the atomic weight be assumed as 13·8, it follows that beryllium belongs, not to the magnesium group, but to that of aluminum, that its atomic weight is 13·8, its specific heat 0·4079, and that beryllium oxide is Be_2O_3 as Berzelius claimed.—*Ber. Berl. Chem. Ges.*, xi, 381, March, 1878. G. F. B.

2. *On a new Synthesis of Olefines.*—ELTEKOFF has succeeded in effecting a new synthesis of hydrocarbons of the general formula C_nH_{2n}, by heating together for seven or eight hours molecular quantities of pentylene (prepared from commercial amylene), and methyl iodide to 210°–215° with an excess of anhydrous lead oxide. The product, freed from lead iodide and methyl oxide, which distilled completely between 36° and 85°, consisted to the extent of one third, of a mixture of C_6H_{12} and C_7H_{14}, boiling from 70° to 83°. This fraction combined energetically with bromine, yielding a solid compound fusing at 139°–140°, volatile with partial decomposition, and having the formula $C_6H_{12}Br_2$. Also with sulphuric acid, a liquid and a solid body separating out on dilu-

tion, the latter having a characteristic camphor-like odor, easily volatile in a current of steam, crystallizing from weak alcohol in long needles fusing at 75°–76° and having the formula $(C_7H_{16}O)_2.H_2O$. It is therefore pentamethylethol hydrate. Fuming hydrochloric acid also gave with this fraction a solid product recalling strongly camphor, boiling for the most part between 120° and 125°, and fusing at 100°. Analysis showed it to be a mixture of $C_6H_{13}Cl$ and $C_7H_{15}Cl$. Hence by the above reaction a hexylene and a heptylene are produced, the structure of which the author is engaged in investigating.—*Ber. Berl. Chem. Ges.*, xi, 412, March, 1878. G. F. B.

3. *Transformation of Olefine bromides into bromides of radicals of the Fatty Acids, by the simple addition of Oxygen.*—Demole, hoping by the direct addition of oxygen to the brominated olefines, to obtain mono, di, or tribrominated derivatives of an oxide of the olefine, made the experiment and observed that dibromethylene in presence of air changed readily into a solid substance. Repeating the experiment with pure oxygen, 50 grams of dibromethylene was placed in a 100 c.c. flask, the air displaced by dry oxygen, the flask closed with a rubber cork and strongly agitated for several minutes. The temperature rose and the cork was strongly pressed inward. This operation was repeated 30 or 40 times until the oxygen was no longer absorbed. The liquid, now at 55° C., was of a clear, yellow color, fumed in the air, was strongly acid, and contained a small quantity of the solid body. On fractioning, a product was obtained boiling at 147°–148°, possessing the formula and all the properties of bromacetyl bromide $(C_2H_2BrO)Br$. The reaction therefore is



$$\begin{matrix} CHBr \\ \| \\ CHBr \end{matrix} + O = \begin{matrix} CH_2Br \\ | \\ C{=}O \\ | \\ Br \end{matrix}$$

On boiling the bromide with water, a crystallized acid was obtained, which was bromacetic acid. Tribrominated ethylene also absorbs oxygen under similar conditions, and yields dibromacetyl bromide.—*Bull. Soc. Ch.*, II, xxix, 204, March, 1878. G. F. B.

4. *On a Remarkable Reaction of Boric acid and Borax with the Polyatomic Alcohols.*—Klein has observed that when a mixture of a solution of borax and one of mannite is made in such proportions that less than half a molecule of borax acts on a molecule of mannite, the resulting liquid is acid. This reaction is not confined to mannite, but occurs also with glycerin, erythrite, levulose, dextrose, α and β galactose, so that a piece of litmus paper placed in the liquid takes a strong onion-red tint, and the mixture itself decomposes carbonates. Moreover, if one of the above named polyatomic alcohols in solution, be mixed with a solution of boric acid, so dilute that it no longer reddens litmus, there is at once developed a decided acid reaction, which turns litmus onion-red. The alkali-earthy borates behave in the same way as borax. In a subsequent paper, Klein gives the results of

his experiments to test the extreme delicacy of the boric acid reaction. If one cubic centimeter of a $\frac{1}{2000}$ solution of boric acid (just perceptibly acid to test paper) be mixed with 10 c. c. of a 15 per cent solution of mannite, a strong acid reaction is developed turning litmus paper onion-red at once. A $\frac{1}{10000}$ solution of borax, which is neutral, when 2 c. c. are mixed with 10 c. c. of the mannite solution, gives a bright-red at once. Five c. c. of a $\frac{1}{100000}$ solution added to 5 c. c. of the mannite solution, gave a decided acid reaction at the end of a minute. Quantitative experiments showed that the alkali required to neutralize the acidity thus produced, was the exact quantity needed to form the monometaborate.—*Bull. Soc. Ch.*, II, xxix, 195, 198, March, 1878.

G. F. B.

5. *On the Products of the Distillation of Resins and resin Acids with Zinc dust.*—CIAMICIAN has studied the products obtained by the distillation of common resin (colophony) and of its acid abietic acid, with zinc dust. The acid was prepared by Maly's method, and was obtained in white crystals. It was mixed, in portions of 20 grams with 10 grams of zinc dust, and distilled in a wide combustion tube in a current of hydrogen. From 600 grams abietic acid, 250 cubic centimeters of distillate were obtained, which on fractioning, gave toluene, metaethylmethyl-benzene, naphthalene, methyl-naphthalene, and methylanthracene. Colophony itself, when thus distilled gave the same products, and in about the same proportion with the exception of toluene, which was present in much smaller quantity. On distilling gum-benzoin with zinc dust, toluene, xylene, naphthalene, and methyl-naphthalene were obtained.—*Ber. Berl. Chem. Ges.*, xi, 269, Feb. 1878.

G. F. B.

6. *On the Triacid Phenols of Wood Tar and on the Synthesis of Cedriret.*—The lower fractions of wood tar having proved so interesting, HOFMANN has undertaken the examination of the fractions of higher boiling point, with especial reference to the beautiful body called cedriret by Reichenbach and cœrulignone by Liebermann. He has succeeded in proving the presence of a whole series of triacid phenols in this portion boiling between 240° and 290°, two of which he now describes. One of these, an oily body, having the formula $C_{11}H_{16}O_3$, forms a crystalline substance with alkalies, and boils at 285°. Treated with acetic oxide, it yields an acetyl derivative $C_{11}H_{15}(C_2H_3O)O_3$, which with bromine gives a bromo-compound $C_{11}H_{13}Br_2(C_2H_3O)O_3$. Heated to 130° with hydrochloric acid, torrents of methyl chloride are evolved, and a crystallized body is obtained having the formula $C_9H_{12}O_3$. This Hofmann regards as a higher homologue of pyrogallic acid, thus $C_9H_9\left\{\begin{array}{l}OH\\OH\\OH\end{array}\right.$; the methyl derivative $C_{11}H_{16}O_3$ being $C_9H_9\left\{\begin{array}{l}OCH_3\\OCH_3\\OH\end{array}\right.$ and its acetyl compound $C_9H_9\left\{\begin{array}{l}OCH_3\\OCH_3\\OC_2H_3O\end{array}\right.$. Since

in the latter body, bromine replaces two hydrogen atoms, the author believes the original body to be the dimethyl ether of a propylated trioxybenzene, or dimethyl propyl-pyrogallate. On oxidation it yields a quinone $C_8H_8O_4$ and this on reduction a hydroquinone $C_8H_{10}O_4$. The second substance obtained from the wood tar was isolated with difficulty and had the formula $C_8H_{10}O_3$. It formed splendid white prisms melting at 51°–52° and boiling at 253°. Concentrated hydrochloric acid heated with it in a closed tube decomposed it, evolving methyl chloride and affording a crystallized substance of the formula $C_6H_6O_3$, which was pyrogallic acid (pyrogallol). Hence the original substance was the dimethyl ether of pyrogallic acid $C_6H_3\left\{\begin{matrix}OCH_3\\OCH_3\\OH\end{matrix}\right.$. By the action of oxidizing agents, most conveniently potassium dichromate, it is converted into the magnificent steel-blue needles of cedriret. In proof of this as the origin of the cedriret of Reichenbach, Hofmann prepared this dimethyl ether synthetically and found that it gave identically the same product on oxidation.—*Ber. Berl. Chem. Ges.*, xi, 329, Feb. 1878. G. F. B.

7. *On the Constituents of Corallin.*—ZULKOWSKY has examined the corallin of commerce and finds it to be a mixture of at least six different bodies: 1st, a coarsely crystalline garnet-red body with blue luster, of the composition $C_{19}H_{14}O_3$; 2d, a derivative of this, in small violet needles, $C_{19}H_{16}O_6$; 3d, a green body crystallizing in needles with a metallic luster, $C_{20}H_{16}O_3$; 4th, the hydro-compound of this, $C_{20}H_{18}O_3$; 5th, a gummy amorphous body, colorless when pure; and 6th, the oxidation product of this, a deep red amorphous powder, showing metallic luster, resembling Baeyer's derivative from phthalidein and phenol.—*Ber. Berl. Chem. Ges.*, xi, 391, March, 1878. G. F. B.

8. *On Curarine.*—SACHS has examined the active substance of curare and finds it to be an alkaloid of the composition $NC_{18}H_{35}$, which he calls curarine. In the curare the alkaloid is combined with sulphuric acid as sulphate. The statements of Preyer he finds to be incorrect. Various reactions of the alkaloid are given in the original paper.—*Liebig's Ann.*, cxci, 254, Feb. 1878.

G. F. B.

8. *Protection of Marine Boilers.*—In marine steam engines with surface condensers, in which the water from the condensed steam is returned directly to the boiler, the boiler plates become rapidly corroded by the fat acids derived from the lubricators, and, moreover, the water condensed from the steam with the usual apparatus—even after filtering through animal black—is unfit for drinking and culinary uses. M. Hétet, chemist to the French marine, has removed these serious inconveniences by the use of lime-water, which he prepares as needed, and injects into the "feed pipe" in regulated quantities by very simple means. The lime soap which forms assumes a granular condition and falls to the bottom of the boiler, from which it is easily "blown out,"

while the water obtained from the steam is perfectly potable. M. Hétet gives evidence that his method has been thoroughly tried with entire success.—*Ann. de Chim. et de Phys.* (5), xiii, 29.

J. P. C., JR.

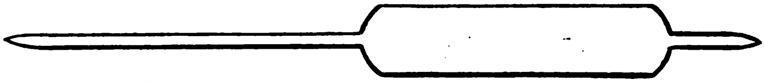
9. *Boracic Acid.*—M. ALFRED DITTE (Ann. de Chim. et Phys., (5), xiii, 67) has made some new determinations of the "heat of solution" of boric hydrate, and also of the "heat of hydration" of boric oxide, which have led to remarkable results. It appears from his experiments that one equivalent (62 grams) of boric hydrate in dissolving in 213 equivalents of water absorbs 3186·7 units of heat, and that one equivalent (35 grams) of boric oxide (fused boracic acid) in combining with water absorbs 6254·7 units of heat, taking as the value in each case the mean of several experiments. It is thus evident that although the heat of hydration is very large the heat absorbed by the hydrate in dissolving is likewise large, amounting to more than one-half of the previous quantity, and hence on dissolving the anhydride in the large mass of water required for its solution the elevation of temperature is not considerable. But if we add to the anhydride in fine powder only a small quantity of water—yet an excess over that required for its hydration—for example, double its own weight, and then stir the mixture, the powder immediately swells while absorbing water, and at the same time so much heat is evolved that the temperature of the mass rises to 100° C., and the excess of water is driven off as steam; the phenomena recalling those so familiar in the slacking of lime. From the specific heat of boric hydrate, M. Ditte calculates that with the exact amount of water required to form the hydrate, and assuming that none of the heat was dissipated, the temperature of the mass would be raised to 283°. M. Ditte finds that for the specific gravity of boric oxide at 12° the value $\delta=1{\cdot}8476$, and at 80° the value $\delta=1{\cdot}6988$. Hence we obtain for the coefficient of expansion between these limits the value $k=0{\cdot}0013$. He also has determined the corresponding values for boric hydrate.

At 0°	$\delta=1{\cdot}5463$	Between 12° and 60° $k=0{\cdot}00154$
12'	$\delta=1{\cdot}5172$	" 12° and 80° $k=0{\cdot}00148$
14°	$\delta=1{\cdot}5128$	
60°	$\delta=1{\cdot}4165$	
80°	$\delta=1{\cdot}3828$	

Further, it appears that the mean density of the boric oxide and ice, which may be regarded as united in the hydrate, is 1·3003, while that of boric hydrate at 0° is 1·5463. There must, therefore, be a considerable degree of condensation in the act of combination, which of course would be attended with evolution of heat. The amount of heat thus evolved is equivalent to that which would be required to expand the boric hydrate sufficiently to reduce its density to 1·300. This amount can readily be calculated from the coefficient of expansion and the specific heat already known, and it is found that the temperature required would be 136° and the amount of heat absorbed 2982·2 units. Evidently then less than one-half of the heat of hydration is due to this cause. M. Ditte has also determined the solubility of boric hydrate at various temperatures, and lastly, he has found the means of crystallizing the hydrate in hexagonal prisms of considerable size which have an easy basal cleavage. The crystals were obtained by the spontaneous evaporation of an aqueous solution acidulated with acetic acid and containing also in solution argentic nitrate. Analysis showed that these crystals were pure boric hydrate. J. P. C., JR.

10. *Boiling Point of Iodide of Antimony.*—In continuation of the writer's investigations on the Haloid Compounds of Antimony, the boiling point of antimonious iodide has been determined by Mr. W. Z. Bennett, student in the chemical laboratory of Harvard College. The observations were made with Regnault's air thermometer, but it was found possible to simplify very greatly the details of the process without seriously impairing the accuracy of the result. For temperatures above the range of a mercury thermometer, measurements accurate to one degree centigrade are all that the uncertain conditions of most problems permit, and all therefore that the circumstances demand. As used by Regnault, the air thermometer is capable of measuring such temperatures accurately to the one-tenth of a degree and by multiplying observations possibly to the one-hundredth of a degree. In his admirable investigation of the boiling point of sulphur at different temperatures, the observations of *temperature* are undoubtedly accurate to this extent, but Regnault's own discussion of these observations plainly indicates that there must have been unknown or accidental causes influencing his experiments, which render the results uncertain to at least one degree, and the boiling point of sulphur is still in doubt to this extent. It should be added, however, that there are only a very few boiling points which are known more accurately; for even when within the range of a mercury thermometer an observation of a boiling point to be accurate to a tenth of a centigrade degree requires an attention to circumstances which is seldom bestowed on such observations.

The glass thermometer bulb used in our experiments is represented in the accompanying figure of one-half the actual size in its linear dimensions. The longer stem was made of fine thermometer tube, and a shorter stem was added to the opposite end of the bulb in order to facilitate the cleaning, drying, filling, or emptying of the interior—all of which was easily accomplished by the aid of a Bunsen pump. The shorter stem was of course sealed after the bulb had been dried and made ready for use, and before it was immersed in the medium whose temperature was to be measured. After an equilibrium had been established at this unknown temperature T°, the protruding end of the longer stem



was sealed, and at the same time the height of the barometer H was noted. The bulb was then taken to a room of uniform temperature provided in the laboratory for gas analysis, and after being mounted on a convenient support, the end of the stem was broken off under mercury, and the apparatus left to itself for a time to secure a perfect equilibrium of temperature. This temperature (T'°) was then observed by means of a standard thermometer hanging near the bulb; also the height (h) to which the mercury had risen in the bulb was measured by a cathetometer, and in addition the height H' of the barometer (hanging in the same room) was again noted. Closing now the open stem with the finger, the bulb was quickly inverted and the containing mercury drawn out into a tared vessel and weighed (nipping off the end of the shorter stem in order to admit the air). This gave the weight w. Lastly, the bulb and stems having been completely filled with mercury by suction, the weight (W) corresponding to their total capacity was obtained in a similar way. The required temperature could now be calculated by means of the following formula, which is easily deduced from the well-known law of Charles:

$$T^{\circ}+273^{\circ}2=(T'^{\circ}+278^{\circ}\cdot2)\frac{W}{W-w}\cdot\frac{H}{H'-h}[1+(T-T')^{\circ}k].$$

It will be noted that as the mercury columns, including the heights of the barometer, were all measured at the same constant temperature, and, as we are dealing with relative values only, no reductions are necessary. Moreover, an error of one-tenth of a millimeter in the value of $\frac{H}{H'-h}$ would make in determining the boiling point of sulphur (448°) a difference of only one-eighth of a degree, so that measurements of these heights are sufficiently close if accurate to one-half a millimeter, and might even be made with a common rule. The most uncertain element in the formula is the expansion of glass, but if the bulbs are made of flint glass (lead glass) tubing, such as is used for ornamental ware, the mean coefficient of expansion will vary very little from 0·000025 if the temperature does not exceed that at which the glass begins to soften. The rate of expansion of flint glass is not only less than that of crown, but it is also more constant and increases very slowly with the temperature. Flint glass is therefore the better adapted for the use we are describing. The expansion of the glass used in our experiments was carefully determined and found to have the value given above, within two or three tenths of a unit in the last place. A difference of one unit in this place would make a difference of one-third of a degree in the boiling point of sulphur.

In order to test the accuracy of the method, Mr. Bennett made four determinations of the boiling point of sulphur under different barometric conditions, which in the following table are compared with the results of Regnault reduced to the corresponding pressures:

Barometer. Height at 0°.	Boiling Point of Sulphur.		
	Bennett.	Regnault.	Diff.
758·8	447°·4	447°·3	+1
763·9	448 ·2	447 ·7	+5
769·6	448 ·2	448 ·1	+1
776·7	448 ·2	448 ·7	−5
	Average difference,		·05

Regnault made eight observations on the maximum tension of sulphur vapor at temperatures varying from 387°·64 to 554°·03, and from a discussion of these deduced the constants of an exponential formula by which he calculated a table of maximum tensions for every ten degrees between the extreme limits, and also plotted a corresponding curve. It so happens, however, that the only two observations within the range of ordinary atmospheric pressure fall outside, and on the same side, of this assumed curve. These observations are the ones usually taken as indicating the boiling point of sulphur, and Victor Meyer in his method of determining the density of the vapors of substances, which have a high boiling point, assumes a value for the boiling point of sulphur (at the mean atmospheric pressure at Zurich) which he obtains by simple interpolation from the two observations just referred to.* In like manner we have calculated the above values corresponding to the presures at which Mr. Bennett's results were obtained on the basis of the same two observations; but instead of simply interpolating by first differences we have assumed that the variation between the two observed values would follow the law indicated by the general curve, which Regnault gives as the best expression for *all* his observations. But, according as we take the two observations, or the whole, we obtain values for the boiling point of sulphur differing by more than a degree, and hence, as we have already said, there is still an uncertainty in regard to the boiling point to this extent. As is evident, Mr. Bennett's observations confirm very closely the interpretation of Regnault's results adopted both by Victor Meyer and by ourselves, but since the boiling point of sulphur has become such an important constant we propose to have the observations repeated under the most favorable conditions we can command.

After the accuracy of our method had been thus placed beyond doubt within the limits required, Mr. Bennett made three determinations of the boiling point of antimonious iodide with the follow results:

Barometer. Height at 0°.	Boiling Point SbI_3.
758·1 millimeters.	400°·4
758·4 "	400°·9
759·3 "	401°·9

Probably only a small part of the differences between these observations depends on the variations of pressure, and 401° is the nearest whole number of degrees to the boiling point of antimonious iodide at the normal pressure of the air.

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* This Journal, III, xiv, 484.

The method we have here described we can most confidently recommend as a most efficient and accurate means of determining high temperatures in chemical laboratories. It requires no expensive apparatus and no more delicate manipulation than most processes of gas analysis. Indeed, this method is most readily associated with Bunsen's methods for gas analysis, like those being most efficiently conducted in a room of constant temperature.

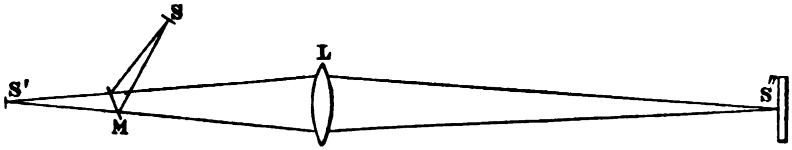
J. C. P., JR.

11. *The Influence of the density of a body upon its Light-absorbing power.*—Professor GLAN has conducted a series of experiments with chemically pure deuto-sulphate of copper, double chromate of potash, solution of iodine in absolute alcohol and solution of iodine in sulphuret of carbon upon the influence of density upon the absorption of light by these substances. He used a photometer, described in Wied. Ann., 1,351, 1877, with which by means of a double image prism, together with a Nichols prism, he could compare the absorption spectra. The author concludes from his experiments with the above mentioned substances that if there is any change of absorption with change of density that it must be very small. He is inclined to think that his results give evidence of this slight change. He also compared the absorption spectra of fluid bromine and bromine vapor and detected a decided change in the absorption coefficient.—*Ann. der Physic und Chemie*, No. 1, 1878, p. 54. J. T.

12. *On reflection of Polarized Light from the Equatorial surface of a Magnet.*—Dr. KERR details his optical experiments at full length upon the above subject. His previous results appeared to him to be confirmed. In the present paper he notices that the intensity of the optical effects of magnetization varies greatly with the angle of incidence. At an incidence of 85° the effects are very faint; between 75° and 60° they are comparatively clear and strong, and at 30° they grow fainter and similiar to the effects noticed at 85°. Dr. Kerr is inclined to believe that the effects are brought out better by reversals of magnetizing currents than by breaking it, in the case of steel. He deduces the following general laws: *First Law.*—The right-handed current conspires with a small right-handed rotation of the analyzer from extinction, and so forward. *Second Law.*—The right-handed current conspires with a small left-handed rotation of the polarizer from extinction, and so forward. These laws are subject to exceptions.—*Phil. Mag.*, March, 1878, p. 161. J. T.

13. *On a method of measuring the Velocity of Light;* by ALBERT A. MICHELSON, Ensign U. S. Navy, Instructor in Physics and Chemistry, U. S. Naval Academy. (From a letter to the Editors.)—The following method of measuring the velocity of light dispenses with Foucault's concave reflector, and permits the use of *any* distance. In the figure, S is a division of a scale ruled on glass; M, a revolving plane mirror; L, an achromatic lens; S″, a fixed plane mirror, at any distance from L.

The point S is so situated that its image S′ reflected in the mirror M, is in one focus of the lens L, while the image of S′ coincides with the mirror S″, which is placed at the conjugate focus. With this arrangement, when M turns slowly, the light from S″ is reflected back through the lens, so that an image is formed which coincides with S. When, however, the mirror rotates rapidly, the position of M will have changed while the light travels from M to S″ and back again, so that the image is displaced in the direction of rotation of the mirror.



Let V be the velocity of light; D, twice the distance M S″; n, the number of turns per second; r, the distance M S and δ the deflection; then V is found by the formula $V = \frac{4\pi r n D}{\delta}$.

In a preliminary experiment the deflection amounted to five millimeters when the mirror revolved 128 times per second.

II. Geology and Mineralogy.

1. *On the Limestones of the Falls of the Ohio*, by James Hall. 16 pp. 4to. Advance sheets of vol. v, part 2, of the Paleontology of New York.—Professor Hall reviews the facts with reference to the beds at the Falls of the Ohio and their fossils, gives the results of personal observations, and arrives at the conclusion that the rocks include—beginning below:

(1.) Niagara beds, of the Upper Silurian, which are the "Catenipora beds" of S. S. Lyon.

(2.) Upper Helderberg beds, to which belong the next following strata, (*a*) Coral beds, (*b*) Turbo bed, (*c*) Nucleocrinus bed, and (*d*) Spirifer bed, of Lyon.

(3.) Then 30 feet of Hamilton beds, which are of impure magnesian limestone, and comprise (*a*) the Hydraulic limestone, and (*b*) the Encrinital limestone, of Lyon.

(4.) The "Black Slate," which, after a special discussion of the facts, is made equivalent to the Genesee shale of New York.

Professor Hall remarks on the point that the Hamilton period is represented at the Falls only by limestones, mentioning the fact that the formation, which is 1,200 feet thick in the eastern part of New York State, and mainly arenaceous and shaly, with calcareous intercalations among the shales at its base, thins down, in 300 miles westward, to a few hundred feet of calcareous shales with some bands of limestone, and that farther west, the limestone gradually becomes still more predominant as the general thickness diminishes; adding that "it is therefore a fair and logical inference that the continuation of this group of strata farther west would preserve the calcareous beds alone."

2. *Report on the Fossil Plants of the Auriferous Gravel Deposits of the Sierra Nevada;* by L. LESQUEREUX. Memoirs of the Museum of Comparative Zoology at Harvard College. Vol. vi, No. 2. 58 pp. 4to, with ten double plates.—Professor Lesquereux here describes fifty species, and gives figures of the leaves on which they are based. He concludes:

(1.) The species are related by some identical or closely allied forms to the Miocene, and still more intimately by others to the present flora of the North American continent.

(2.) The North American facies is traced by some species to the Miocene, the Eocene, even the Cretaceous of the Western Territories. Hence it is not possible to persist in considering the essential types of the present North American flora as derived by migration from Europe or from Asia, either during the prevalence of the Miocene or after it. This flora is connatural and autochthonic.

(3.) The relation of the Pliocene plants of Nevada and Tuolumne Counties is with the flora of the Atlantic slope, and not with that of California at the present time. This fact is explained by the influence of glacial action during the prevalence of the ice-period, and is even clearly exposed by the distribution of the few Pliocene species remaining in the flora of the Pacific coast. The modification of the characters of the present flora of California have, therefore, to be looked for in climatic or other phenomena subsequent to the Glacial period. This remarkable fact, so clearly demonstrated by nature, may serve as an exemplification of the causes of the disconnection of some of the other groups of our geological floras.

3. *Memorandum of a fossil wood from the Keokuk formation, Keokuk, Iowa;* by SAMUEL J. WALLACE, of that place.—A portion of a supposed fossil wood was found in the Keokuk formation, Subcarboniferous, at Keokuk, Iowa, March 6th, 1878.

It was a section nearly three feet long; one end disappearing in the bluff, and the other having apparently been taken off in the quarry years ago. It is flattened into a layer resembling lignite, from $\frac{1}{16}$ to $\frac{1}{6}$ inch thick, and twelve inches across; divided into two portions with a space of about an inch between, and not quite on the same apparent plane. It does not seem to have external or bark markings, but rather those of woody fiber, possibly exogenous, which, however, has not been made out to any degree of certainty.

It was obtained in a quarry in the bluff beside the railroad track fronting the levee, nearly three blocks below Main street, from the best quarry layers of the Keokuk Formation, five feet below the "Geode bed," and from the center of a solid 18-inch limestone layer, on a horizon of numerous shells, fish teeth, etc. Is not this among the first distinct land plants from this formation?—*From a letter to the Editors.*

4. *Atlas accompanying the Report of the Geological Exploration of the Fortieth Parallel;* by CLARENCE KING, U. S. Geologist

in charge. Made by authority of the Hon. Secretary of War, under the direction of Brig. and Brevt. Major-General A. A. Humphreys, Chief of Engineers, U. S. A. 1876.—One of the maps of this Atlas has already been noticed in this Journal in volume xi (page 161). The completed atlas has recently been issued. It is in very large folio, and consists of ten double maps (nearly twenty maps of page size) illustrating the geology and topography of a region nearly fifty miles wide either side of the fortieth parallel from western Nevada to eastern Colorado. The atlas is a grand contribution to the Geology of the continent, and bears testimony to the very great care and thoroughness of the surveys under Mr. King. The five colored geological maps present not only the distribution of the several areas of igneous, granitic, and stratified rocks, but even of the principal kinds of igneous rocks—the rhyolites, trachytes, leucite rocks, porphyry, andesite, diorite and diabase, the study of which in the field, by Mr. King, has been supplemented by the exceedingly valuable volume of descriptions by Professor Zirkel, forming part of the Reports of the Survey.

The plates are from the establishment of J. Bien, New York, and are admirable specimens of chromo-lithography.

5. *Geological and Geographical Atlas of Colorado, and portions of adjacent Territories;* by F. V. Hayden, U. S. Geologist in charge, U. S. Geological and Geographical Survey of the Territories, Department of the Interior. Large folio.—This Colorado Atlas, just issued, is another very important contribution toward a general geological chart of the western half of the American Continent. It embraces twenty maps each of the size of two of the folio pages; a triangulation map from the work of J. T. Gardner of 1873, 1874, 1875, and subsequently of A. D. Wilson; a drainage map; an "economic" map giving the distribution of forests or groves of different kinds, pasture land, agricultural land, sage land, coal land, gold districts and silver districts; a general geological map; six sectional geological maps, and as many topographical, on a scale of four miles to the inch; two maps of geological sections and one of panoramic views. The areas of the various rock formations, including the subdivisions for the kinds of igneous rocks, are distinguished by colors. The map engraving and chromo-lithography are by J. Bien, as with the preceding atlas, and they show the same beautiful work in each respect, the harmony and choice of tints being all that could be desired.

6. *A Monograph on the Genera Zethus, Cybele, Encrinurus and Cryptonomus*, by A. W. Vogdes, U. S. Army. 36 pp. 8vo. Charleston, South Carolina.—This memoir is a historical and descriptive review of the genera, with descriptions of the species of Cryptonomus, illustrated by four plates, photographed from foreign memoirs, representing the following species: *C. punctatus*, *C. variolaris*, *C. bellatulus*, *C. obtusus*, *C. lævis*, *C. vigilans*, *C. sexcostatus*, *C. nereus*, and *C. verrucosus*. The limits of the genus are those accepted by Eichwald in 1840. It includes *Encrinurus*, and *Cybele* and *Atractopyge* are made subgenera under it.

7. *Carboniferous and Upper Silurian fossils of Illinois and Indiana.*—Dr. C. A. White has published descriptions of some invertebrate species in the Proceedings of the Academy of Natural Sciences of Philadelphia for 1878, p. 29.

8. *Die Culm-Flora der Ostrauer und Waldenburger Schichten* von D. Stur. 366 pp. 4to.—An elaborate memoir on the fossil plants of the Lower Carboniferous formation of Moravia, illustrated by twenty-seven large and beautiful plates, a map, and two plates of sections. The lower Culm includes the Carboniferous limestone with *Productus giganteus* Sow., occurring in Altwasser, Neudorf near Silberberg, Hausdorf, etc., with which the "Culm-Dachschiefer," containing *Posidonomya Becheri* Br. is equivalent; and the upper Culm, the Ostrau and Waldenburg shales. Some of the specimens of plants figured are of remarkable size and interest.

9. *Remains of the Musk Ox* (*Ovibos moschatus*) have been found in the lœss of the Rhine near Unkel, according to F. Rœmer.—*Zts. geol. Ges.*, xxix, 592.

10. *Notice of three new Phosphates from Fairfield County, Connecticut*;* by George J. Brush and Edward S. Dana.—(1). Eosphorite. Usually observed in prismatic crystals whose obtuse angle measures 104½°, and which probably belong to the orthorhombic system. The crystals are uniformly terminated by two pyramids in different vertical zones. Cleavage macrodiagonal nearly perfect. Also commonly compact massive. Hardness=5. Specific gravity =3·132–3·145. Luster vitreous to greasy. Color pink, some crystals having the bright shade common in rose-quartz, while others are paler and have a yellow to gray hue. The compact mineral is pale pink, greenish and bluish white and white. Some varieties closely resemble in color and luster green elæolite; the green color, however, is probably due in all cases to the presence of impurities. Transparent to translucent. Before the blowpipe in the forceps cracks open and fuses at about 4 to a black magnetic mass. Dissolves completely in the fluxes, and gives reactions for manganese and iron. Soluble in nitric and hydrochloric acids. Analyses by S. L. Penfield have proved it to be a hydrous phosphate of manganese, iron and aluminum, with the atomic ratio of $P_2O_5 : Al_2O_3 : RO : H_2O = 1 : 1 : 2 : 4$. This corresponds to the empirical formula $Al_2R_2P_2O_{10}, 4H_2O = AlP_2O_8 + 2H_2RO_2 + 2Aq$. The compact varieties contain quartz and other impurities. An analysis of a whitish compact specimen by Horace L. Wells gave 14·41 of insoluble impurities, and the remainder had the atomic ratio of eosphorite.

(2.) Triploidite. Occurs in crystalline aggregates whose structure is parallel-fibrous to columnar, also divergent, and again confusedly fibrous to nearly compact massive. Isolated prismatic crystals are occasionally observed imbedded in quartz. In very rare cases these crystals have been detached with their terminations preserved; the crystallographic data thus obtained show that the mineral is closely related in form to wagnerite. Hard-

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* Communicated by the Authors.

=4·5–5. Specific gravity=3·697. Luster vitreous to greasy-
iantine. Color yellowish to reddish brown, the crystals
sionally topaz- to wine-yellow. Transparent to translucent.
s in the naked lamp-flame, and B. B. in the forceps colors the
pale green. Completely soluble in the fluxes, giving reac-
for iron and manganese. Soluble in nitric and hydrochloric
Analyses by S. L. Penfield have proved it to be a hydrous
phate of manganese and iron, giving the atomic ratio of
: RO : H_2O of 1 : 4 : 1. This corresponds to the formula
O_5, $H_2O=R_3P_2O_8+H_2RO_2$. The mineral in external charac-
as a marked resemblance to triplite, and this fact is expressed
e name which has been given it.

) Dickinsonite. Occurs foliated massive; often lamellar
te, the laminæ being sometimes straight, but more often
ed. In one instance observed in tabular crystals with stri-
base. These have a rhombohedral aspect, but are shown by
tical examination to be twins belonging to the orthorhombic
nonoclinic) system, the prismatic angle being about 120°.
vage basal, perfect; folia very brittle. Hardness =3.5–4.
ific gravity =3·338. Luster vitreous, on the cleavage face
what pearly. Color oil to olive-green, sometimes approach-
rass-green. Transparent to translucent. In the closed tube
tens, fuses and gives off water having a faint acid reaction.
re the blowpipe in the forceps fuses at 1 to a black magnetic
ale, and colors the flame pale-green with an occasional faint
of red. With the fluxes gives reactions for iron and man-
se. Soluble in acids. The chemical analysis now in progress
L. Penfield indicates it to be a hydrous phosphate of iron and
anese with alkalies, the spectroscope showing the presence
th soda and lithia.

e above species are from a deposit of manganese minerals in
n of coarse albitic granite which has been quarried for mica.
ciated with them is a considerable amount of a ferriferous
ochrosite which, according to Mr. Penfield's analysis, contains
t 27 per cent of carbonate of iron. There are also present:
ck phosphate of iron manganese and lithia, a purple phos-
resembling heterosite, and a black hydrated oxide of man-
se; all these apparently are products of the oxidation of the
minerals. We have also determined the presence of vivi-
, hebronite, apatite, and some other phosphates whose com-
ion needs further investigation before a final conclusion in
d to their specific character. It is our intention to publish in
uly number of this Journal a full description of the locality.
will include the results of the crystallographic and optical
ination of the new species, the chemical analyses already
, with others still in progress, and all the facts in regard to
nethod of occurrence which we may obtain from the addi-
l supply of material we expect soon to receive.

Mineralogy: Vol. I, *The General Principles of Mineralogy;*
H. Collins, F.G.S. 206 pp. 8vo. New York, 1878. (G. P.

Putnam's Sons—Putnam's Advanced Science Series).—This work is intended primarily "for practical working miners, quarrymen, field geologists, and students of the science classes." The first volume now issued (the second being in the press) contains a clear and systematic description of the physical and chemical properties of minerals. The system of Miller is principally used in the chapters upon crystallography, though the symbols of Naumann are also given. The value of this portion of the work is enhanced by the very large number of figures of crystals, which will do much to remove the inherent difficulties of the subject.

III. BOTANY AND ZOOLOGY.

1. *Synoptical Flora of North America;* by ASA GRAY.—The first part of this work is expected to be published before May-day. It will form a bound volume of itself, with index, etc.; but it is only the first part of vol. ii. The publication begins with vol. ii, because it takes up the American Flora where the Flora of North America by Torrey and Gray dropped it five and thirty years ago, viz: at the end of *Compositæ.* The ground which was covered by that work, newly worked over, will be comprised in the first volume of the new work. The present publication includes all the *Gamopetalæ* after *Compositæ*, and fills over 400 compact imperial 8vo pages. The price is fixed so as barely to recover the cost of the edition. For this sum ($6), it will be sent by mail to any part of the United States, post-paid, if ordered from the *Curator of Harvard University Herbarium, Cambridge, Mass.* In the trade it is published by Messrs. Ivison, Blakeman, Taylor & Co., New York.

2. *Bibliographical Index to North American Botany; or Citations of Authorities for all the Recorded Indigenous and Naturalized Species of the Flora of North America, with a Chronological Arrangement of the Synonymy;* by SERENO WATSON. Part 1. POLYPETALÆ. Washington: Smithsonian Institution, March, 1878, 8vo, 476 pp. including table of Orders, and index to orders and genera.—This forms No. 258 of the *Smithsonian Miscellaneous Collections;* and surely it will be ranked among the most useful volumes of a valuable series. What a formidable undertaking it is, what a vast amount of pains-taking and critical labor has been bestowed upon it, may be imagined by turning over the well-filled pages, but will be adequately understood only by working botanists. To such it will be a great boon; and thankful will they be to the venerable Secretary of the Smithsonian Institution that he should have taken this work in hand, so rendering its production possible. Not less grateful should they be to the editor for his patience, conscientious thoroughness, and disinterested zeal, which, as in similar instances, can be recompensed only by the consciousness of having done useful work, very helpful to fellow-laborers and those who are to come after them. The Secretary, in his advertisement, states that this work "is published by the Smithsonian Institution, at the request of the leading bot-

ists of the United States, who have also contributed to the pense of its preparation." But the contributions must have en few and of comparatively small amount, while the labor has en protracted.

The portion now issued covers the ground of the first volume Torrey and Gray's Flora of North America, i. e. of the Polyetalous Dicotyledons, and may therefore be regarded as a comlete portion or volume. It is very elaborate bibliography, but t mere bibliography or index-making, for which simple painsking might suffice; but it is a critical revision and re-investigaon of the whole ground, not only supplementing that volume of e Flora by all that has been published since, but abounding in w and independent determinations and judgments. All the pographical arrangements and execution are excellent. To ing this volume well within the reach of all who need it, the ice is fixed at two dollars, in paper covers. Those who need it ost will much prefer bound copies. We may be useful by anouncing that the editor has had a small number of copies put to strong cloth binding; and that these may be obtained, from e curator of the Harvard University Herbarium, at the addional cost, viz: at $2.25, if early application is made. A. G.

3. *On some points in the Morphology of Primulaceæ;* by M. Masters, M.D., F.R.S., etc.—A paper in the first volume of he new series of the Transactions of the Linnean Society of ondon (in which Botany and Zoology are separate), based upon review of the principal monstrosities known in Primroses, and other plants, so far as they throw light upon these; illustrated y three plates, crowded with figures; followed by a full selection the bibliography of the subject. The placenta of *Primulaceæ* concluded to be "a direct prolongation of the receptacle or xis, without any connection with the sides or apex of the carels." "Yet in some monstrous flowers, placentæ are outgrowths ther from the margin or center of carpels; these outgrowths ay become detached, and may cohere with one another, thus proucing the appearance of a solid column directly produced from e receptacle. Such forms of placenta lead to the inference that e ancestral progenitors of *Primulaceæ*, had parietal placentaon." It may be added that the whole does not militate more gainst present carpellary placentation in *Primula* than in *Dionæa.* he ovular integuments are concluded to be foliar. The explaation of the anteposition of stamens to corolla-lobes and of the ter appearance of the corolla is not at all settled. A. G.

4. *On the Origin of Floral Æstivations, etc.;* by Rev. G. enslow, M.A., F.L.S.—A paper in the same volume of Linn. rans. The forms of æstivation are classified under eight equivaent kinds, five of which we should regard as species or varieties the imbricative; the term *convolute* (in place of contorted) is eartily adopted for one of them, following the suggestion in his Journal; the degrees of frequency of the various kinds are dicated; the mode in which they may pass into each other is hown; their origination under evolution speculated upon with

ingenuity; the cruciferous flower is conceived to have arisen symmetrical reduction of fives to fours, etc.; the hypothesis tl the corolla of *Primula* is an outgrowth of the andrœcium controverted, more particularly on the ground that the devel ment of the parts of normal flowers is by no means always c tripetal. A. G.

5. *Floral Structure and Affinities of Sapotaceæ;* by Marc M. Hartog, M.A., etc.—A short paper in Trimen's Journal Botany, for March, 1878. Observations made at the Bota Garden at Peradeniya, Ceylon, and very neatly worked out. . to the ovules, "the impression" is that they are the axillary bu of the carpels. Flowers almost always proterogynous. Evo tion in the flower "strictly centripetal, with a tendency to au mentation in the number of parts in the whole as we advan from the periphery inwards." "Each new member arises front of the widest intervals between the next oldest membe If the intervals be wide, the new members are formed in front the widest intervals between the members of the next old whorl and those of the next but one," both falling under H meister's generalizations. The order nearest to *Sapotaceæ* is *My sinaceæ;* and *Styracaceæ* (*Symploceæ* being separated) nearer th *Ebenaceæ.* Finally the "outgrowth" theory of the petals *Primulaceæ* is opposed by the inference that the origin a structure of the corolla must be essentially the same in this ord as in *Myrsinaceæ;* that "in many plants the petals, though fi formed, are soon overtaken and outstripped by the stamens. Car the delay a stage further back, and you have the history of *Pri ulaceæ.*" The paper closes with an amended character of *Labo donnaisia*, and of an allied new genus *Eichleria*, in honor of o of the best morphological botanists of the day, Dr. Eichler, wl —let us add our congratulations—succeeds to the chair vacated l the death of Alexander Braun at Berlin. Professor Schwenden from Tübingen, takes the new chair of Physiological Botany Berlin. A. G.

6. Curtiss: *North American Plants.*—The first part of t collection of dried plants of our Southern States (250 specie which Mr. A. H. Curtiss announced a year or more ago, is n issued. The set supplied to the Harvard University Herbari enables us to declare that the specimens are well chosen, copio and perfect, are carefully put up, all named, with printed tick in neat form and taste; and that these sets are cheap at the pri viz: twenty dollars for 250 species. To favor this laudable ent prise and to facilitate their acquisition by botanists, some se have been deposited at the Harvard University Herbarium, tl Curator of which will receive applications for them. A. G.

7. *On the Spore-Formation of the Mesocarpeæ, and especially the new Genus Gonatonema;* by Dr. V. B. Wittrock.—In tl genus *Gonatonema*, founded on *G. ventricosum*, a new speci and the old *Mesocarpus notabilis* of Hassall, the cells bend in something like a knee-joint, and divide into three parts, in tl central one of which the spore is formed directly, without conj

. Dr. Wittrock observed a rotation of the chlorophyll-
previous to the formation of the spore. The writer calls
ion to the fact that in ***Mougeotia calcarea*** he has found that
ores are sometimes formed in three different ways, sup-
to be characteristic of the three different genera, ***Meso-***
, ***Plagiospermum*** and *Staurospermum*. W. G. F.

Von-Sexual Outgrowths on Fern Prothalli.—The discovery
non-sexual production of the Fern-plant from the pro-
s, by Dr. Farlow, when a pupil of De Bary at Strasburg,
en extended to numerous instances. The facts and bearings
case were reported to the Society of German Naturalists,
late meeting at Munich, by Professor De Bary, as follows:
stigation has shown that some Ferns, namely, *Pteris Cre-*
Aspidium falcatum, and *Aspidium Filix-mas*, var. *cris-*
, form on the prothalli normal antheridia but generally no
gonia, or imperfect ones which perish before the opening of
nal, and exhibit instead of the typical formation of the em-
the outgrowth described by Farlow. In those species which
p archegonia no such outgrowth has been observed. The
ological phenomena presented by the shoot are alike in
ecies named, even in the smallest particulars. In the pro-
s which grows directly out of the spore, the development
occurs in most cases, and which may be called normal, is as
s: at the sinus, nearly at the spot where the first archego-
occurs, there arises a protuberance which grows out directly
leaf, at the base of this, close by the insertion of the pro-
s, there is formed an axial *punctum vegetationis*, on which a
l and the successive later leaves appear. At the base of
st leaf there is formed, endogenously on the vascular bundle,
st root. As soon as the second leaf appears, the bud grows
n ordinary fern-shoot. Variations differing in degree from
rmal mode of growth are not uncommon. Frequently the
lli form branches similar to themselves (secondary prothalli)
can again produce leaves and shoots in a great variety of
A moment's reflection shows that the three Ferns in ques-
ave lost the power of forming archegonia, and with it sexual
luction, and as compensation for the sexual formation of the
o, possess the power of forming the outgrowth described
rlow. This presents a special case of that general phenom-
which is called *apogamy*, total loss of reproductive power,
hich consists in the fact that a species loses sexual repro-
n and receives in its place numerous non-sexual modes of
gation, such as brood-buds, suckers, etc. The numerous bulb-
the higher phanerogams, species of *Allium*, *Dentaria*, and
e, are examples of this, as well as of the successive degrees
erence in apogamy." Closer observation of the phenomena
roduction in apogamous plants other than Ferns was recom-
d. Dr. Farlow's paper was originally published in the
edings of the American Academy of Arts and Sciences,
G. L. G.

9. *A Catalogue of the Flowering Plants and higher Cryptogams growing without cultivation within thirty miles of Yale College.* Published by the Berzelius Society, New Haven, 1878, pp. 72, 8vo, and an outline map.—This catalogue has reached us barely in time for announcement here. Its form and typography are attractive, and the editors (whose names are not mentioned), appear to have done their work admirably, under the auspices of Professor Eaton, who adds an introduction, with interesting historical details. The catalogue extends to the *Musci* and *Hepaticæ*, and the summary of species gives the total number of 1,506.

A. G.

IV. ASTRONOMY.

1. *Der Sternhaufen* χ *Persei*, etc., von Dr. H. C. VOGEL. Leipzig, 1878. 4to, pp. 36.—Dr. Vogel, of the Potsdam Observatory, has published the results of measures of the cluster χ *Persei*, made in 1867–70 by means of the 8-inch refractor at Leipzig, with the object of fixing the relative positions (and magnitudes) of the stars of this cluster, so that any future change may not pass undetected. 176 stars, in all, have been fixed in position by the filar-micrometer. The field was bright, and a magnifying power of 145 diameters was employed throughout. The various sections of this work of 36 quarto pages treat of the following subjects:

§ 1. *The position of the instrument; the determination of the parallel.* A novel feature here is that the zero of position was determined before and after each set of measures (either of p and s or of $\Delta\alpha$ and $\Delta\delta$) on each pair of stars, so that the observations for any pair are arranged thus: 1st, four measures of p;. four measures of s; 2d, determination of parallel; the next pair is then observed as follows: 1st, four measures of p; four measures of s; 2d, determination of parallel; and so on throughout the night.

§ 2 contains an *investigation of the position-circle;* and of the *value of the revolution of the micrometer.* The zero of the micrometer was found to be dependent upon the position of the instrument and also upon the kind of illumination of the threads. The value of the revolution is found from transits on 20 nights, from Nov., 1867, to May, 1870. During all this time the reticle was left at the same distance from the objective, and the thermometric coefficient resulted $+0''\cdot001581\ t^{\circ}$ in Reaumer's scale. The magnitude and the *sign* of this, Dr. Vogel explains by the fact that the focal point was not determined each night (as he says is usual), and he correctly points out the necessity of leaving the focus unchanged for such observations and for determining the value of the screw during the series itself.

§ 3 deals with *the methods of observation and reduction.* The brighter stars less than 10th magnitude were determined from measures of p and s with four selected stars of the group. These four were connected by measures of p and s and also $\Delta\alpha$ and $\Delta\delta$; and they were further connected with two stars of the cluster *h Persei*, which had been observed with the Bonn meridian-circle. The pairs of stars were observed alternately on different threads, the zeros determined under the same circumstances as those of the observations, the position angles measured by turning the circle in both ways. From four to six measures of p and s were made each night, and for each pair of the brighter stars at least four nights observations were made, i. e., at least sixteen measures of p and s. The reductions are complete, and the observations are reduced to 1870·0.

The fainter stars (10–12 mag.) were observed by $\Delta\alpha$ and $\Delta\delta$ with other stars at least on two nights for each star.

§ 4 deals with the *accuracy of the observations;* for the brighter stars, the probable error of a *single observation* is found to be, in *s*, ±0″·228, in *p* (reduced) ±0″·306. The probable errors for the *mean of each night* are more important, and result as follows (no dependence of the probable errors on the distance or the position-angles being evident), probable error of one night in *s*, ±0″·190, in *p* (reduced) ±0″·165. For the final position (at least four nights) these become, in *s*, ±0″·092, in *p*, ±0″·080. For the $\Delta\alpha$ and $\Delta\delta$ of the brighter stars these are ±0″·097 in R.A. ±″·089 in N.P.D. The positions for the fainter stars are determined within less than 1″ in each coördinate, which Dr. Vogel considers sufficient for his purpose.

§ 5 treats of the *determinations of the brightness of the stars of this cluster.* The 176 stars of the cluster range between the 6·5 and 13th magnitudes.

Each one of the fainter stars (higher than 10 mag.) was determined by eye estimates of magnitude at least 5 times; the probable error of the mean is ±0·14 magnitude. The brighter stars were determined on several evenings by the eye, and on two nights each was compared by a Zollner's photometer with one of the standard stars. A table (p. 12) gives the magnitudes of the brighter stars, 1st, by eye (Vogel); 2d, by eye, (Argelander); 3d, photometric magnitude, assuming the light ratio $\frac{1}{2\cdot518}$ or 0·397. The agreement is remarkable, but the table shows (what was already known) that Argelander's magnitudes higher than $9\cdot0^m$ make the stars too bright.

§ 6 gives the *Observations of the stars* (in tabular form) *and the Results.* A difference between the Spring and Autumn observations, in *both* $\Delta\alpha$ and $\Delta\delta$, of one of the stars indicates possibly a parallax of about 0″·3.

§ 7 gives the *Observations of the Fundamental Stars; and Catalogue of the* 30 *brightest stars.* The observations are of relative $\Delta\alpha$ and $\Delta\delta$ of the four fundamental stars, and of two of Argelander's stars in *h Persei* and also meridian observations. These last also indicate a parallax to the star *b*. None of the stars appear to have a large proper motion.

§ 8 deals in the same way with *Observations of the fainter stars; and Catalogue of all the stars of the cluster.* This is followed by two charts, one of the brighter stars and the plan of triangulation, the other of the whole cluster.

This brief analysis will give an idea of the contents of this extremely thorough paper, which will take its place beside the other researches of the author in the same field. They are all models of what such investigations should be, and leave nothing to be desired in methods of observation or reduction, in the accuracy of the final results reached (which are always adequate to the purpose in hand), and, finally, are excellent examples of the literary style and clearness appropriate to such memoirs.

E. S. H.

2. *American Journal of Mathematics Pure and Applied.* Published under the auspices of the Johns Hopkins University. Vol. i, No. 1. 4°, 104 pp. Baltimore, 1878.—We are glad to greet the first number of this quarterly. It fills a place in American Journals that has been too long vacant. The primary object of the Journal is to publish original investigation. In addition, concise abstracts of subjects to which special interest may attach, and critical and bibliographical notices and reviews of the most important mathematical publications form part of the plan.

The present number contains contributions from Messrs. Newcomb, Hill, Eddy, Weichold, Cayley, Rowland, Peirce, Sylvester. The two longest articles are by Mr. G. W. Hill, *Researches in the Lunar Theory*, and Professor Sylvester, *On an application of the new Atomic Theory to the graphical representation of the Invariants and Covariants of Binary Quantics.*

3. *Publications of the Cincinnati Observatory.*—No. 4 of this series contains a table of micrometric measurements of Southern Double Stars, covering 72 pp. royal octavo, made during the year 1877 by Messrs. Herbert A. Howe and Winslow Upton, and the Director of the Observatory, Prof. Ormond Stone.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Driftless Region of Wisconsin.* (Communication to the Editors.)—There are one or two sentences in Professor Dana's paper on the "Driftless Interior of North America," in the April number of the Journal, which call for remark from me, since their effect is to credit me with more than I deserve in the matter of the theoretical conclusions as to the glacial phenomena of the northwest, given in volume ii of the Geology of Wisconsin. As the same error occurs in other reviews of our work, it is evident the matter is not made as plain as it should be in the reports themselves. I give, therefore, a brief statement of the facts. In 1874, Professor T. C. Chamberlin investigated the already known Potash Kettle Range of Eastern Wisconsin, and came to the conclusion that it was a gigantic moraine produced by the separate glaciers of the Green Bay and Lake Michigan troughs. Subsequently he traced the range much farther southward than before known, found it curving westward and northward again, and carried it to the southern boundary of the district under my charge. In June, 1875, he furnished me with a map

on which he had marked the probable position of the continuation in the Central Wisconsin District. Afterward I verified this and located the range as mapped in my report; and in studying out my own observations on the Glacial Drift, found them to harmonize so thoroughly with his theory that the Kettle Ranges are continuous moraines, that I yielded to his idea and used it in explaining the phenomena observed in my own district—among them the existence of the Driftless Region. The honor of the first recognition of these great moraines as such, and of their great value as indicators of the positions and size of the various glaciers, and of glacial movements generally, must be given entirely to Professor Chamberlin. Should this idea stand the test of investigation, it will, beyond doubt, lead to some important conclusions.

ROLAND D. IRVING.

2. *A Treatise on Chemistry;* by H. E. Roscoe, F.R.S. and C. Schorlemmer, F.R.S., Professors of Chemistry in Owens College. Vol. I. The Non-metallic Elements. 769 pp. 8vo. New York: 1878. (D. Appleton & Company).—This treatise supplies a want which every teacher and student of chemistry feels; of a clear succinct statement of the facts of modern chemistry, fairly complete and with such a discussion of chemical theory as the present state of the science permits within the proper scope of such a work.

3. *Report of the Superintendent of the Coast Survey for* 1874. —This volume contains, among its very valuable articles, the elaborate paper in full of Mr. Charles A. Schott (Assistant in the Coast Survey) on the Secular change of Magnetic Declination in the United States and other parts of the United States, and also another by J. E. Hilgard, Assistant, on the determinations of Transatlantic Longitudes of 1872, being the final Report, giving a review of previous determinations.

4. *Minnesota Academy of Natural Sciences.*—The Bulletin for 1877 contains a catalogue, with notes, of the Mycological flora of Minnesota, by A. E. Johnson, M.D., occupying 100 pages.

5. *Matter and Motion;* by J. Clerk Maxwell, M.A., LL.D., etc. 224 pp. 12mo. New York, 1878. (D. Van Nostrand—Van Nostrand Science Series.) This essay is reprinted from the April and May numbers of Van Nostrand's Magazine. It is a very clear statement of some of the fundamental principles of physics by a most eminent authority.

6. *Contributions to North American Ethnology: Tribes of California;* by Stephen Powers. Department of the Interior, U. S. Geographical and Geological Survey of the Rocky Mountain Region, J. W. Powell in charge. 636 pp. 4to, with a map and several 4to plates.—The work treats with fullness of the people and all their characteristics, and the plates illustrate their forms, features, music, habits, weapons, implements, etc. Many of the pages of the volume are occupied by vocabularies from various sources, edited by Prof. Powell.

Report of the Chief of Engineers for the year 1877. Parts I and II. 1456 pages, 8vo. 1878.

OBITUARY.

DR. CHARLES PICKERING.—Dr. Pickering died in Boston on the 17th of March, of pneumonia, at the age of seventy-three. He was a grandson of Col. Timothy Pickering, who was a member of Washington's cabinet and one of the most distinguished men of his day. He was a member of the class of 1823 at Harvard College, and graduated from the Massachusetts Medical School in 1826. He practiced medicine several years in Philadelphia, and while there devoted much of his time to the Academy of Natural Sciences of that city.

In 1838, Dr. Pickering was appointed one of the Naturalists of the United States Exploring Expedition under the command of Charles Wilkes, U. S. N. The expedition gave him good opportunities for pursuing his favorite studies. And these opportunities for original observation were further enlarged; for, soon after his return from this voyage, on the 11th of October, 1843, he left for Egypt, Arabia, India, and the eastern part of Africa, in order to continue his investigations with regard to the races of men and the distribution of plants and animals. Again at home, he published, in 1848, The Races of Men, and their Geographical Distribution, being volume ix of the Exploring Expedition. In 1854, he had ready for the press The Geographical Distribution of Animals and Plants, being volume xv of the United States Exploring Expedition; but, through the withholding of government appropriations, only the first portion of this valuable Report has been published. Another large work, entitled Man's Record of his own Existence, was only recently completed, and is now passing through the press.

Dr. Pickering was untiring and most conscientious in scientific research, and of great and varied learning. His works are repositories of carefully observed facts on the subjects he had laboriously investigated, along with the conclusions to which he had been led.

With all his learning, he had neither a desire, nor a gift, for display. He was a man of the finest moral sensibilities, and the most perfect integrity, engaged during a long life in absorbing studies, yet asking neither fame nor money, nor any other reward than the privilege of gaining knowledge and storing it up in convenient forms for the service of others. He loved his friends, and was a most genial traveling companion, though naturally reserved among those with whom he was not familiar.

Dr. Pickering was married, in 1851, to Sarah S., daughter of the late Daniel Hammond, Esq., of Boston, and leaves no children. His memory will always be dear to those who knew him; his works should secure for his name a high place in the annals of American Science. J. D. D.

APPENDIX.

ART. LIX.—*Notice of New Fossil Reptiles*; by Professor O. C. MARSH.

THE United States Survey of the Fortieth Parallel, in charge of Mr. Clarence King, has made known the fact that a well marked Permian horizon can be distinguished in the Rocky Mountain region; and deposits considered of this age are represented on the geological maps of that survey. This adds much interest to the vertebrate fauna known from near this horizon, and probably belonging to it, as hitherto no Permian vertebrates have been identified in this country, although not uncommon in Europe.

The Museum of Yale College contains an extensive series of Reptilian remains belonging to a peculiar lacustrine fauna, which includes also Amphibians and Fishes. These fossils are from several localities in the West, but mainly from New Mexico, and the geological horizon appears to be in the upper portion of the Permian. These Reptilian remains are in excellent preservation, and among them are several genera, having the more important characters of the *Rhynchocephala*, of which the genus *Hatteria*, of New Zealand, is the living type. The principal points of agreement are the separate premaxillaries; the immovable quadrate; and the biconcave vertebræ. Another character of much interest is the presence of certain hypaxial elements of the vertebræ, first observed by Von Meyer in the Triassic genus *Sphenosaurus*, and called by him intercentral bones, ("*Zwichenwirbelbein*"). These wedge-shaped bones are apparently the homologues of the cervical hypapophyses in the *Mosasauria*, and of the sub-caudal attachments in the *Odontornithes*, and a few recent birds. These intercentral ossifications apparently exist in all the Reptilia yet found in this new fauna, and hence serve to distinguish it. With this character is another of hardly less interest. The anterior rib-bearing vertebræ preserved have three separate articular facets for the ribs; one on the anterior part of the centrum for the head, and a double one above for the bifid tubercle. In the implantation of the teeth and their successional development, these Reptiles resemble the Mosasuria.

These characters, with others mentioned below, indicate two distinct families, which may be called *Nothodontidæ* and *Sphenacodontidæ*, from the typical genera here described.

Nothodon lentus, gen. et sp. nov.

This genus of Reptiles may readily be distinguished by the dentition. In each separate premaxillary there are two slender pointed teeth. In front of the maxillary there are one or two similar teeth, followed by a number with narrow transverse crowns, resembling in form the premolars of some carnivorous mammals. These crowns, when unworn, have a central cusp, and on each side a tubercle, somewhat like that on the premolars of the genus *Canis*. In the present species the first and last of the transverse teeth are smaller than the middle ones. The limbs were short, the long bones had their extremities covered with cartilage, but the carpals and tarsals were well ossified. The centra were very deeply concave, and the tail was long.

The following measurements are taken from the type specimen of this species:

Length of maxillary bone	65·$^{\text{mm}}$
Space occupied by ten maxillary teeth	55·
Height of crown of second maxillary tooth	14·
Height of crown of third maxillary tooth	9·
Antero-posterior diameter	3·
Transverse diameter	8·
Antero-posterior diameter of eighth tooth	5·
Transverse diameter	15·

The present species was about five or six feet in length, and herbivorous in habit. It was apparently slow in movement, and probably more or less aquatic. The remains at present known are from New Mexico.

Sphenacodon ferox, gen. et sp. nov.

In the present genus the anterior teeth are somewhat like those of the reptile described above, but the posterior, or more characteristic ones, are totally different. The crowns are much compressed, and have very sharp cutting edges, without crenulations. In the present species the carnivorous teeth are crowded together, and the crowns placed slightly oblique, and twisted. The jaws were comparatively short and massive. The rami of the lower jaws were apparently united by cartilage only, and the symphysis was short. The vertebræ are deeply biconcave.

Measurements from the type of this species are as follows:

Length of dentary bone	150·$^{\text{mm}}$
Space occupied by teeth	130·
Extent of four anterior caniniform teeth	25·
Extent of twenty compressed teeth	105·

Height above jaw of second lower tooth	15·mm
Depth of dentary bone at symphysis	26·
Height of crown of compressed tooth	8·
Transverse diameter	4·

This reptile was about six feet in length, and carnivorous in habit. Its remains are from the same locality in New Mexico that yielded those of *Nothodon.*

Ophiacodon mirus, gen. et sp. nov.

A third genus of Reptiles allied to the last described is indicated by various well preserved remains from the same locality. The teeth are all carnivorous in type, conical in form, and all are similar. Those in the anterior part of the jaws are recurved, and in general shape resemble those of Serpents. The rami of the lower jaws were united only by cartilage. The vertebræ are very deeply biconcave, and even perforate, and the intracentral bones large. In the present species the teeth are nearly smooth, and somewhat compressed.

The following measurements indicate the size of this reptile:

Extent of anterior sixteen teeth in dentary	75·mm
Extent of anterior five lower teeth	20·
Height of crown of fourth lower tooth	10·
Depth of lower jaw at symphysis	15·
Extent of seven anterior maxillary teeth	33·
Height of crown of first maxillary tooth	9·
Antero-posterior diameter of crown	3·

This species was about as large as those described above, and is from the same geological horizon in New Mexico.

Ophiacodon grandis, sp. nov.

A second larger species of apparently the same genus is represented by portions of the jaws, and teeth, and various parts of the skeleton. In this species the dentary bone is angular at its anterior extremity, and triangular in section. Its external surface is rugose, as in the Crocodiles. The crowns of the teeth are striate at the base, and the latter is furrowed vertically. The teeth are not so thickly set as in the smaller species, and the bases of the crowns are somewhat transverse.

Measurements.

Space occupied by ten anterior lower teeth	140·mm
Depth of lower jaw at symphysis	129·
Antero-posterior extent of symphysis	25·
Depth of dentary bone below seventh tooth	30·
Width of dentary at this point	20·

The present species was about ten feet in length, and the largest reptile yet found in this fauna. The remains are from New Mexico.

Yale College, New Haven, April, 1878.

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ART. LX.—*On the Transmission of Sensation and Volition through the Nerves.** *Contribution from the Physical Laboratory of the Cornell University;* by M. M. GARVER, B.S., Professor of Natural Science in Mercersburg College, Pa.

DURING the winter of 1875–6 some experiments were made in the Physical Laboratory of the Cornell University, under the direction of Professor Wm. A. Anthony, to determine the rate of transmission through the nerves. The experiments were continued for a period of several months, during which some interesting and, as far as the writer's knowledge goes, new results were obtained.

The apparatus used for measuring the time was a modified form of the Schultz Chronograph and consisted essentially of a tuning-fork making 128·1 vibrations per second, a metallic cylinder covered with smoked paper, and an induction coil for giving and recording signals. The cylinder was capable of being rotated freely about a horizontal axis by means of a handle. The prolonged axis of the cylinder was cut with a screw and worked in a fixed nut; consequently when the handle was turned the cylinder gradually advanced in the direction of its axis. The tuning-fork, which was kept in vibration and regulated by one of König's automatic break-pieces, bore upon one of its prongs a flexible style of brass which was placed in contact with the blackened paper. Then when the tuning-fork was set to vibrating and the cylinder turned, an undulating line was traced spirally round the cylinder; each one of the undulations corresponding to a known interval of time. The base of the tuning-fork was connected with one wire from the induction coil and the cylinder with the other, so that when the primary circuit was broken a spark would pass from the end of the style through the paper to the cylinder, displacing the lampblack in its passage and leaving a small dot in the undulating line or trace.

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*This article consists principally of extracts from a paper presented by the author for the degree of B.S. at the annual commencement of the Cornell University, June, 1876. For that reason the original name is retained, although the matter relating to "volition" has been almost entirely omitted.

The method of using the apparatus was as follows: the wire of the secondary circuit connected with the tuning-fork was conducted to one side and used to give the signals by placing a short break in the wire in contact with the part of the body desired, the flesh of the body completing the circuit. The resistance in the primary circuit was so regulated that the signal could be distinctly felt without being painful. Then in the primary circuit two keys were necessary, one for the operator with which to give the signal and one for the individual experimented upon with which to answer the signal. The operator's key was so arranged that the circuit could be broken and closed again by simply depressing the lever. The answering key consisted of a light vertical lever, capped with a small insulating knob of ivory having a groove in its upper surface into which the nail of the index finger was placed in order that the circuit could be broken with the least possible lost motion, by simply flexing the finger. All the contacts of the keys were of platinum.

When everything was in readiness, the operator turned the cylinder and depressed his key; the spark passed, giving the signal and dotting the paper. As soon as the signal was perceived, the finger was flexed, another spark passed and was also registered upon the smoked paper. Then the time between receiving and answering the signal was found by counting the number of vibrations which were registered between the two dots,—the fractions of vibrations being estimated in tenths by the eye.

It was found that the time required to perceive and answer a signal even when given at the same point, varied considerably; consequently large numbers of observations were necessary in order to attain to anything like accuracy in the results. The first series of observations were taken from the cheek and foot, and the difference in time as computed from the means was 0·0412 of a second. The distance was about five and a half feet, consequently the velocity was about 133·7 feet per second. Afterwards, however, more careful measurements were made, and the difference in time found between the foot and hip, hand and shoulder, neck and upper lumbar region, sight and hearing, and the probable errors of the results computed according to the usual method for such cases.

In experimenting upon myself the time required to answer a signal given on the left hand, as deduced from one hundred and nine single observations, was found to be 0·1572″±·0009″; when the signal was given upon the shoulder, the time as obtained from sixty observations, was 0·1482″±·0010″. The difference is 0·009″ and the distance, as measured, twenty-three inches, hence the velocity from the data, was in this case 213·± 28·6 feet per second.

The results can probably best be shown in a table (see table I.) All the series from which these results were obtained are not given because they would swell the proportions of this article to an undue extent; and also because the establishment of the reliability of the numerical results obtained, it is expected, can be shown to be of minor importance.

TABLE I.

Person experimented upon.	No. of observations.	Point of application of signal.	Time required to answer.	Difference in time.	Length of nerve.	Velocity in feet per second.
Fowler (1),	30	Left foot,	0·1707″	·0412″	5½ ft.	133·7
	46	Left cheek,	0·1296			
Garver (2),	109	Left hand,	0·1572″ ± ·0009″	·009″	23 in.	213 ± 28·6
	60	Left shoulder,	0·1482″ ± ·0010″			
Lee (3),	53	Left hand,	0·1686″ ± ·0017″	·01546″	21 in.	113·2 ± 17·4
	51	Left shoulder,	0·1532″ ± ·0017″			
(4),	103	Left foot,	0·1866″ ± ·0014″	·0111″	36 in.	270·7 ± 45
	105	Left hip,	0·1755″ ± ·0012″			
(5),	95	Neck,	0·1508″ ± ·0009″	·0130″	16½ in.	105·5 ± 10·1
	90	Back,	0·1638″ ± ·0008″			
(6),	84	Left foot,	0·1927″ ± ·0014″	·0098″	36 in.	307·4 ± 47·6
	93	Left hip,	0·1829″ ± ·0010″			
(7),	210	Left hand,	0·15425″ ?	·00003″	23 in.	60000· ?
	196	Left shoulder,	0·15423″ ?			

It will be noticed that the time in (4) and (6) differs considerably in the two sets of observations, although taken from the same person. The difference can hardly be owing to errors in observation, for the sum of the probable errors is less than ·003″, while the differences referred to are ·0061″ and ·0074″. There was evidently a change in the conducting power of the nerve, or in the mental status of the individual during the interval of time which elapsed before the last series were taken. Number (4) was taken Jan. 8th; number (6) a week later.

Number (7) gives an extraordinary result. It is so very large that it can hardly be regarded otherwise than as erroneous. Doubtless there was a change in condition while taking the observations, for all the observations from the hand were taken before any were taken from the shoulder, and the fatigue incident upon taking so large a number of observations is considerable, so that it can hardly be claimed that the experiments were made under exactly the same conditions. If, however, the observations had been taken alternately from hand and shoulder, any change in condition would have affected both alike, and although the mean time might have increased or diminished, the differences would not have been materially affected. It is unfortunate that the order of the observations was not preserved, for then the whole series could have been broken up, and the change, if any, detected. This series will be again referred to hereafter.

Experiments were also made to determine the relative time required for receiving sensations through the eye and ear. Table II exhibits the results obtained from four different individuals.

TABLE II.

Name.	Time from "sight to hand."	Time from "ear to hand."	Difference in favor of the ear.
Lee	0·1628″ ± ·0011″	0·1327″ ± ·0009″	·0301″
	0·1793″ ± ·0012″	0·1359″ ± ·0019″	·0434″
Garver	0·1856″ ± ·0015″	0·1651″ ± ·0016″	·0205″
Brown	0·1808″ ± ·0035″	0·1364″ ± ·0020″	·0444″
Blake	0·1809″ ± ·0018″	0·1439″ ± ·0015″	·0370″

It will be noticed that the time required to hear a sound is in all cases *less* than that required to see a light; and the difference is sufficient to allow a sound to traverse a distance of from twenty to thirty feet and still be perceived as soon as a light emanating from the same point at the same instant.

In determining the time from "ear to hand," the induced spark from the coil was made to pass in close proximity to the ear, and the time registered as in the other cases. In finding the time from "sight to hand," it was necessary to eliminate the sound of the spark, and in order to accomplish this a small glass tube was fitted with platinum wires so as to give a spark about an inch in length, and partially exhausted of air. If the exhaustion be properly regulated, the spark is sharp and distinct, but perfectly noiseless.

It would seem that experimenters in attempting to determine the velocity of nervous transmission, have generally assumed that under the same circumstances, the rate of transmission is constant. Let us analyze a few series of observations and see what light they throw on the subject.

If the rate be constant, or if the determination of the rate involve no other elements than those found in measuring an ordinary fixed magnitude, the observations when plotted for the purpose of determining the accidental errors, should give a curve approximating to that shown in fig. 1, which is the well-known curve of probability. And inversely, it must be

1.

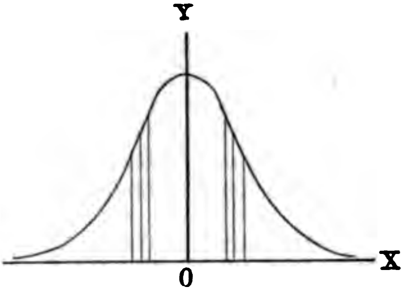


Fig. 1. *The curve of probability.* (From (Chauvenet.) Constructed from the equation $y=\frac{1}{\sqrt{\pi}}e^{-\Delta\Delta}$, where *e* is the base of the Napierian system of logarithms, *y* the *probability*, and Δ (i. e., *x*) the value of the errors. The scale of the ordinates is made four times that of the abscissæ in order to show the character of the curve more distinctly.

evident that if the "errors" are *not* accidental, but are governed by some other law, the form of the curve will vary accordingly, and may serve to give a clue to the new law.

The readings were taken in vibrations and tenths, the value of which was determined once for all. Let us take as examples two series where the signals were given on the left foot and answered as in every case by flexing the index finger of the right hand. The number of times each number appeared was as follows, the sign = being used instead of the word "appeared:"

Fowler.
I.

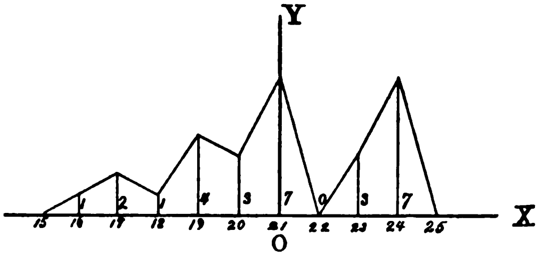
16=1
17=2
18=1
19=4
20=3
21=7
22=0
23=3
24=7
25=0
26=0
27=0
28=1
29=0
30=1

Fowler,
II.

16= 2
17= 4
18= 4
19= 7
20=10
21= 7
22=1
23=8
24=2
25=1
26=2

There were tenths in many instances, but for the sake of brevity they have been omitted.

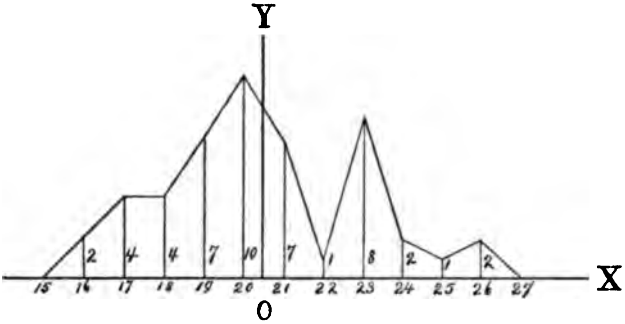
2.



Now if the curves representing these observations be plotted, they will be seen to have two or three maxima and minima, and evidently come under the second case. (See figs. 2 and 3).

The two principal maxima differ by three vibrations, i. e., by about $\frac{1}{50}$ of a second. And here is another peculiarity to which attention is particularly called, for it may serve to explain the anomalous results obtained in some particular cases. The maxima in I (fig. 2) occur at 21 and 24, while in II (fig. 3) they appear at 20 and 23, showing an evident change in the condition of the nerve. The observations in II were taken the day after those in I, otherwise the conditions were apparently the same. It is impossible to tell when the change took place or to tell the cause, but if such a change should occur during the time occupied in taking the observations, it is manifest that the results would be materially affected and the peculiar *periodicity* of the results more or less destroyed.

3.



A few more examples showing a periodicity will be given, but instead of plotting out in curves, it will be sufficient to indicate the groups by braces. The numbers at the right indicate approximately the difference between the means of the groups; 2½ vibrations being equivalent to a little less than $\frac{1}{50}$ of a second.*

III. Lee. "Sight to hand."		IV. Brown. "Sight to hand."		V. Garver. "Ear to hand."		VI. Blake. "Sight to hand."		VII. Lee. "Hand to hand."		VIII. Lee. "Shoulder to hand."	
19= 2		19= 1		17= 6		17= 1		16= 0		12= 1	
20= 4		20= 4		18= 2		18= 1		17= 2		. . . .	
21=10		21= 1	2½	19=11		19= 0		18= 1		16=2	
22=16		22= 5	vib.	20=14		20= 0		19=10		17=9	
23=12		23= 4		21= 9	3	21= 1		20=11	2½	18=9	2½
24= 4	?	24= 1		22= 6	vib.	22= 3	2	21= 6	vib.	19=5	vib.
25=10		25= 0		23= 7		23= 1	vib.	22=10		20=9	
26= 1		26= 1		24= 5		24= 6		23= 5		21=6	
27= 1		27= 2		25= 0		25= 1		24= 4		22=7	
28= 1		28= 1		26= 0		26= 1		25= 3		23=2	
				27= 2		27= 1		. . . .		24=1	
				28= 1		28= 0		30= 1		25=0	

---

* The exact rate of the fork during these experiments was 128·1 vibrations per second. Previous to the last experiments, in Dec., 1876, the fork underwent some repairs, after which its rate was 127·8.

The difference between VII and VIII is very marked, and by simple inspection, noticing the position of the maxima, we can determine pretty closely the difference in time between hand and shoulder. It appears to be two vibrations, corresponding very closely to the difference between the means, which was found to be 1·98 vibrations.

In the same way let us examine the experiments which gave the anomalous results before mentioned.

IX. Lee.
"Hand to hand."

14= 0	23=10
15= 5	24= 6
16=10	25= 5
17=28	26= 4
18=40	27= 1
19=51	28= 1
20=22	. . . .
21=14	31= 1
22=12	

X. Lee.
"Shoulder to hand."

14= 3	19=32	5 vib.
15= 6	20=25	
16=11	21=21	
17=28	22= 7	
18=34	23= 7	
	24=11	
	25= 7	2½ vib.
	26= 1	
	27= 3	

In X the series shows a tendency to break up into three groups, the means of the first two differing by something over five vibrations, corresponding to about $\frac{1}{23}$ of a second; and this peculiarity will be sometimes noticed, that is, if the interval $\frac{1}{23}$ of a second does not appear, an interval of *twice* that length may occur. In IX, however, no period is observable, and the series corresponds to the theoretical form of a series of observations to determine the value of a *fixed* magnitude. It has been shown how this might occur by the combination of series each periodic in its character; but whether this is so or not in this case, it is impossible to say. The means obtained from these two series are very nearly equal, and gave the doubtful results previously mentioned.

But it may be urged that the periods here shown are not very marked and may be caused by some imperfection in the apparatus employed. In that case I would refer to experiments made with entirely different apparatus, where the measurements depend upon other methods. A few examples out of the many which occur in Dr. Burckhardt's* work will be given. The readings are in millimeters and correspond to ·01 of a second; two millimeters then, being nearly equivalent to two and a half vibrations. The signal was a tactile one given by a light lever, and required in answer the movement of a stipulated muscle. The following, with the exception of the XIV, are taken from pp. 59 and 60.

The numbers in heavy type are those from which he estimated the "norm" or mean. The periodicity is evidently the same as that obtained in our experiments, and must have its foundation in the same cause.

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* **Burckhardt:** Die physiologische Diagnostik der Nervenkrankheiten, Leipzig, 1875.

XI. Right foot.	XII. Left foot.	XIII. Right hand.	XIV. Ear.	XV. XII dors. vert.
5=1	9=1	10=4	16=1	10=3
9=2	10=5	11=3	17=3	11=8 }
10=3	11=4	12=9	18=4	12=4 } $\frac{1}{50}$
11=4	12=5	13=10 }	19=7	13=8 }
12=5	13=8	14=8 }	20=4 }	14=3 }
13=9	14=4 }	15=3 } $\frac{1}{25}$	21=3 } $\frac{1}{25}$	15=8 } $\frac{1}{50}$
14=3 }	15=10 } $\frac{1}{50}$	16=2 }	22=2 }	16=4
15=10 } $\frac{1}{50}$	16=7 }	17=8 }	23=4 }	17=3
16=7	17=8 } $\frac{1}{50}$	18=3	24=1	18=3
17=7	18=0	19=2		19=0
18=4	19=4			
19=3				

From the many examples of the occurrence of these periods, it would appear that they have their origin in the physical or mental action of the individual, but their physiological or psychological significance is not known, and the complete elucidation can be hoped for only through the aid of more extended investigations.

In December, 1876, another method was tried by which it was hoped that results having smaller probable errors could be obtained and at the same time throw light upon the periodicity of previous results. The experiments failed to fulfill expectations principally, perhaps, on account of the imperfect working of the apparatus, but nevertheless some important facts were learned. I am confident that the same or a similar method cannot fail to give interesting results if proper apparatus be employed.

In all the previous experiments the signals were given at *irregular* intervals so as to avoid anything like rhythm, thus requiring an act of perception and an act of volition for each observation; and from what has been shown, it appears that these two acts cannot be performed with any great degree of regularity. In order then, if possible, to obviate this difficulty, the signals were made to follow each other as nearly as may be with perfect isochronism. The method used was this:

With the same registering apparatus previously described, two induction coils were used, one of which in connection with the pendulum of a clock was made to give the signals at intervals of a second, each beat of the clock giving a signal which was registered upon the smoked surface. The other coil in connection with the answering key was used to register the answers. Two coils are necessary because when but one is used the answer is also felt and interferes with the rhythmical order of the signals. Part of the difference in time between the registered signals and answers must evidently be due to the delay caused by the passage of sensation through the nerve, and, if the rate of transmission is uniform, will be proportional to the length of the nerve.

The first attempt was made by simply trying to beat time with the index finger of the right hand to signals given on the back of the left hand. Great care was taken to prevent any idea of the time being received *except through the nerves experimented upon.* On examining the register after the first set of observations was taken, it was found that the answers sometimes followed and sometimes preceded the signals and did not have the regularity expected. Upon noticing that the signal had been frequently anticipated, I made a new effort, taking great pains to be sure that the signal was *felt* each time before answering. A few observations were then taken, and after stopping a short time to examine the effect, a few more were taken. The following was the result, the tuning-fork making 127·8 vibrations per second.

XVI.	
36	33
38·5	28·4
44	32·4
30	33·7
35	35·5
38·8	33·5
33·5	
Mean=34·8	

XVII.	
46	45·2
43	49
31·2	42·7
40	42·5
41	43·5
36·5	42
43	49
49	42·5
36·2	47·4
Mean=42·8	

Here then, we have two sets of observations taken within a few minutes of each other, the external conditions to all intents and purposes exactly alike, yet differing from each other in a marked degree; the mean of the second (XVII), exceeding the mean of the first (XVI) by nearly 25 per cent. The difference is probably due, at least in part, to an error in judgment or inability to recognize the exact instant at which the signal was given.

XVIII.
32
35
29·8
33·7
34
35·4
35·6
37·5

XIX.	
42	31·7
44·5	32·4
44·2	27·3
40·5	23·7
38	25
31·3	26

Two more sets (XVIII and XIX) taken two days afterwards, under as nearly as possible the same external conditions, exhibit another phase. Here it is seen that XIX, taken a few minutes after XVIII, commenced as on the previous day, with a marked increase over the preceding values, but gradually decreased nearly one-half in comparatively few observations.

Not obtaining very satisfactory results on account of the imperfect working of one of the coils, which failed to register, the investigation was broken off at this point. The imperfection mentioned could not affect the accuracy of the results obtained; the coil would simply cease working. It gave "accurate results or none at all."

There are too few observations in any one of the last series, to give any marked indication of the periodicity noticed in the results obtained by the first method; but nevertheless, series XVII, on analysis, shows a tendency to break into three groups, differing by six and a half vibrations. Thus:

XVII.
Garver "Hand to hand."

$$\overbrace{\begin{array}{l}31=1\\ \text{- - - -}\\ \left.\begin{array}{l}36=2\\ \text{- - - -}\\ 40=1\\ 41=1\\ 42=4\end{array}\right\}6\tfrac{1}{2}\text{ vib.}\end{array}\qquad\qquad \left.\begin{array}{l}43=3\\ 44=0\\ \left.\begin{array}{l}45=1\\ 46=1\\ 47=1\end{array}\right\}\\ 48=0\\ 49=3\end{array}\right\}6\tfrac{1}{2}\text{ vib.}}$$

It is very improbable, to say the least, that certain values should be *selected* and certain others be rejected in this way without some cause beyond that of mere accident.

The period, it will be noticed, differs somewhat in different individuals but is almost constant in the same individual. The doubling may be caused by the obliteration of one of the normal groups, or as it appears sometimes, by the rejection of the intermediate values.

It may not be amiss to suggest an explanation of the "periodicity" however liable it may be to be overthrown by further investigation.

It seems that when an individual is experimented upon as in the given cases, he is conscious of being *surprised* by the signal even when expecting it. And sometimes the surprise is such that he forgets to answer until he is conscious of considerable time elapsing. At times he has to "think twice" before he moves his finger or stipulated muscle.

My own experience is that even when concentrating the utmost attention upon the point of application of the signal, I was sometimes aware that the signal was not answered as soon as it might have been; while at other times no appreciable time appeared to pass before answering. Knowing then, that there is a variable element entering after cousciousness, it might not be going too far to assume that the variation is entirely cerebral.

Could not such a periodicity have its origin in the transformation lying between sensation and volition? It is readily conceivable that such might be the case and be of such a nature as to resemble an "increment to the judgment."

Mercersburg, March, 1878.

ART. LXI.—*The Upper Devonian Rocks of Southwest Pennsylvania;* by JOHN J. STEVENSON, Professor of Geology in the University of New York.

THE Vespertine or Pocono sandstone of the Pennsylvania Survey is a massive sandstone from 350 to 450 feet thick in southwestern Pennsylvania, and is the lower division of the Lower Carboniferous rocks. In Fayette and Westmoreland Counties, under Laurel and Chestnut Ridges, the last of the Alleghany Ridges toward the west, the Vespertine sandstone rests immediately upon a mass of gray to reddish-gray sandstones, interstratified with red to gray and olive shales.

These, representing the upper portion of the Devonian, are well exposed in the gaps of the Conemaugh River through the two ridges, as well as in the similar gaps made by the Youghiogheny River. In Laurel Ridge there are a few imperfect exposures in the deeper gorges made by the larger streams within Westmoreland and northern Fayette. In Chestnut Ridge there are no exposures of the Devonian in Westmoreland County, but in Fayette, south from the Youghiogheny River, these rocks are shown in the gap made by Dunbar Creek, as well as on the National Road, and in the deep gorges made by Shute's Run, Redstone Creek and the principal tributaries to Sandy Creek.

Lithologically, the transition from the Vespertine or Pocono sandstone to the Devonian rocks is sufficiently distinct. The great sandstone breaks down into a sandy shale, interstratified with some argillaceous shale, which in turn becomes merged into the well-defined red-gray to olive shales and sandstones representing the Upper Devonian. During the hasty examination of 1876, I was unable to make any close study of the section, and so provisionally regarded the lower rocks as belonging to the same series with the upper. This conclusion was given in my report to Professor Lesley for 1876. But the examinations made in the several gaps during 1877 showed the previous conclusion to be erroneous, and that the lower portion of the section from the very base of the Pocono sandstone is Devonian and not Lower Carboniferous.

A general section of the Devonian rocks, as observed in the gaps mentioned above, is as follows:—

1.	Shales and thin gray sandstones	80 feet.
2.	White to reddish-gray sandstones with some shale	70 "
3.	Reddish-gray micaceous sandstones with red to gray and olive shales	150 "
4.	Red to gray shaly sandstones with variegated clays and shales	200 "
	Total	500 "

Lithologically, the top portion, No. 1 of the section, is a transition mass, more closely related to the overlying than to the underlying rocks. But its relations are clearly shown by the fossils which occur in it. This part of the section is well exposed in the several gaps referred to, as well as on the National Road as it winds up the western side of Chestnut Ridge in Fayette County. At all localities examined it shows the same character, the sandstones are light-gray to brown and in thin beds, while the shales vary from brown to dull blue.

By far the greater part of the rest of the section consists of shale and shaly sandstone, in almost equal proportions and in alternating layers, from two to four feet thick. The sandstones are exceedingly micaceous, and, on the Conemaugh River, are for the most part little more than a compact micaceous mud; but in both gaps of the Youghiogheny there are compact gray sandstones, good enough to be used for building purposes. On the National Road, ten or twelve miles south from the Youghiogheny River, the shale and micaceous sandstones re-appear as on the Conemaugh. These micaceous sandstones are reddish-brown, have a concretionary structure and for the most part break down readily on exposure to the weather. Characteristic fossils are found throughout the section and many of the harder layers have their upper surfaces covered by a close mat of fucoids.

A curious conglomerate, from ten to twenty feet thick, occurs near the middle of the section and seems to be persistent, having been seen under Chestnut Ridge on the Conemaugh and Youghiogheny rivers as well as on the National Road. It is very much like that at the base of the Vespertine or Pocono sandstone, but the pebbles are not flattened and they are much larger than those seen in any other conglomerate exposed within southwest Pennsylvania. They are oval, thoroughly rounded and polished as by long rolling in water. Most of the larger pebbles are quartz, but with them are others of felsite-porphyry, quite soft, which had been blackened exteriorly before they were embedded in the material cementing the mass.

Relations of these Rocks.

In the final report of the First Geological Survey of Pennsylvania, Formation IX, the red Catskill of New York, is mentioned as occurring in the district under consideration. Following that report, I intimated in my second annual report to Professor Lesley that the rocks described in this article might be referred to that formation; on the maps accompanying my third annual report, now passing through the press, the areas are colored as Catskill. This, which was done to preserve unity in the maps of the survey, is not in full accord with the facts.

To determine the relations of rocks one may be guided by lithological characters and relative position, or if possible he may trace the rocks to some typical locality, or should fossils be present he may make his determinations by means of those. For the most part, geologists are satisfied to abide by the last test, as it is of universal application and saves a great expenditure of time and labor. But some geologists are disposed to think the simpler method inaccurate, and seem inclined to rebel against an imagined assumption on the part of paleontologists. It is desirable then to ascertain whether or not the relations of these rocks can be determined by tracing or by lithological characters.

The bold anticlinal axes of southwest Pennsylvania are the Alleghany Mountains, Negro Mountain, the Viaduct axis, Laurel Ridge and Chestnut Ridge, all mountainous for the greater portion of their extent within the State of Pennsylvania. Under these axes alone may one look for exposures of the lower strata, for away from them the surface rocks belong to the Coal Measures.

An exposure under the Alleghanies in Maryland reaches below the Pocono or Vespertine sandstone, but northward there is no described exposure anywhere on the west side of those mountains in Somerset County of Pennsylvania; and, as far as can be ascertained from the report of Mr. Platt's close survey, the deepest gorge on that side is cut down only to the rocks of Formation XI, the Umbral. But in Cambria County, which is immediately north from Somerset, the exposures extend below Formation X, the Vespertine.

Negro Mountain separates itself from the Alleghany Mountains in northern Maryland and passes through Somerset County of Pennsylvania, dying out in southern Cambria, as may be learned from the reports of the Messrs. Platt upon those counties. No exposure in this ridge extends below Formation X.

The Viaduct axis separates itself from the Negro Mountain in northern Maryland and continues as a strong axis through Somerset, Cambria and Clearfield Counties of Pennsylvania. But it nowhere shows anything below the upper portion of Formation X, as appears from the reports made by Messrs. F. and W. G. Platt.

Laurel Ridge, at the line between Pennsylvania and West Virginia, exposes only the upper portion of the Umbral, XI, but at the Youghiogheny River, the upper part of the Devonian is reached. its section being exposed by the railroad cuts. Here and there, northward from the Youghiogheny, a deep gorge is cut down to the Devonian, but owing to the thick coat of debris, no exposures occur and no section can be obtained south from the Conemaugh River. The fold declines north from that river, so that the gaps made within Cambria County by Chest and Black Lick Creeks reach barely to Formation X, and no gap in Clearfield County, south from that of the west branch of the Susquehanna, seems to expose any lower rock. These facts are gathered from the reports of Messrs. F. and W. G. Platt on Cambria, Somerset and Clearfield Counties, and from my own careful observations in Fayette and Westmoreland.

Chestnut Ridge first shows the Devonian rocks near the National Road in Fayette County, but thence northward the axis diminishes in strength, a given stratum being fully 1,000 feet lower at the Conemaugh than at the National Road. Between that road and the Youghiogheny River, several gorges are cut down to the Devonian, but no section can be obtained until the Youghiogheny River is reached. North from the river, owing to the decline of the axis in that direction, the deepest gorges soon fail to reach the Devonian and no exposure exists between the Youghiogheny and the Conemaugh. North from the Conemaugh the fold still decreases in strength, as is well shown by the fact that the Lower Coals creep constantly higher up its sides, so that the gaps made by Black Lick and other streams cannot do more than barely to reach Formation X, especially since the great Conglomerate of XII thickens very materially in that direction, as abundantly appears from the report on Clearfield County by Mr. Franklin Platt.

There is no exposure whatever for more than fifty miles along the west slope of the Alleghany Mountain; no exposure occurs in Negro Mountain or the Viaduct axis, so that no exhibition of Devonian Rocks appears between the Alleghanies and Laurel Ridge, a distance of twenty-five miles in an east and west direction; there are three exposures within seventy-five miles along Laurel Ridge, the intervals being forty and thirty-five miles; while in Chestnut Ridge there are three exposures within sixty miles, the intervals being ten and thirty-five miles. Surely under such circumstances one may hesitate before accepting any conclusion based on mere stratigraphy.

But is lithology any better? At all exposures to which reference has been made, except those in Clearfield County, respecting which I have no knowledge, rocks more or less similar in appearance are found immediately below Formation X, which is believed to represent the gray Catskill of New York. As they are at the top of the Devonian, they are likely to be

Catskill or Chemung, or to represent both groups, unless indeed those have thinned out. Professor H. D. Rogers thus describes the Chemung and Catskill of Pennsylvania:—

"VERGENT SERIES.

"VERGENT FLAGS (*Portage flags of New York*).—A rather fine-grained gray sandstone in thin layers, parted by thin alternating bands of shale. It abounds in marine *vegetation*. Thickness in Huntingdon 1,700 feet.

"VERGENT SHALES (*Chemung group of New York*).—A thick mass of gray, blue and olive-colored shales, and gray and brown sandstones. The sandstones predominate in the upper part, where the shales contain many fossils. Thickness in Huntingdon 3,200 feet.

"PONENT SERIES.

"PONENT RED SANDSTONE (*Catskill group of New York*).—In its fullest development this is a mass of very thick alternating red shales with red and gray argillaceous sandstones. It has very few organic remains. Among them is *Holoptychius*, and one or two other remarkable fossil fishes, of genera distinctive of Old Red Sandstone. This formation has its maximum thickness in its southeastern outcrops, where it measures more than 5,000 feet."—*Final Rep. First Geol. Surv. Penn.*, vol. 1, p. 108.

On pages 140, 141 and 142 of the same volume, Professor Rogers gives some further details respecting the lithological characters of the rocks. In the northwest belt, the Vergent or Portage flags consist of dark gray flaggy sandstones parted by thin layers of blue shale, with large marine plants and a *Nucula* as the chief fossils, while in the next belt toward the west they are made up of thin-bedded, fine-grained, siliceous gray sandstones, intimately alternating with blue and greenish shales.

In the middle belt, the Vergent Shales or the Chemung consists of gray, red to olive sandy shales, with gray and red argillaceous sandstones, but no details are given respecting this group in the belts west or northwest from the Alleghany Mountains.

In the northwest belt, the Ponent or Catskill consists of fine and argillaceous sandstones, with an increase of red and green shale and with some calcareous layers.

On page 793 of vol. ii of the same report, Professor Rogers points out the similarity between the deposits of the Ponent and Vergent, and states that the sediments of the former are quite as impalpable as are those of the latter.

If all these descriptions be compared with those already given of the rocks occurring in the gaps of the Youghiogheny and Conemaugh through Laurel and Chestnut Ridges, it will be seen that, as far as lithological characters are concerned, those rocks may be either Catskill or Chemung, though indeed the evidence seems to be rather in favor of their being Chemung, for if one wished to describe them briefly and comprehensively, he could do no better than to combine Professor Rogers' descriptions of the Portage and Chemung, thus:—

"A rather fine-grained gray and brown sandstone in thin layers parted by alternating bands of gray, blue, olive and red shales. It abounds in marine vegetation, and in the upper part the shales contain many fossils."

Since it would be excessively difficult to determine the relations of these rocks by mere stratigraphy, and since the lithological characters fail to throw any distinct light upon the matter, the third test must be employed.

What are the fossils?

In the Summer of 1877, while making examinations in the Conemaugh Gap through Chestnut Ridge, I found, almost midway in the section given on another page, numerous specimens of *Spirifer Verneuilii*, *Rhynchonella Stephani* and *Streptorhynchus Chemungensis*, associated with many lamellibranchs and poorly preserved brachiopods, which could not be determined at the time. Further examination showed that these species occur up to within eighteen inches of the undoubted Pocono sandstone, or Formation X. The same species were found in abundance on the National Road as well as in the Youghiogheny Gaps; and, at all localities, the harder layers at from 100 to 150 feet below that sandstone are covered by a thick mat of fucoids, many of which have very thick stems and are several feet long.

In order that no doubt might remain respecting these species, I sent some specimens to Professor Hall, who has made out the following list;—

1. *Lingula*, sp.; 2. *Discina grandis* or *D. Alleghaniensis;* 3. *Streptorhynchus Chemungensis;* 4. *Rhynchonella Stephani;* 5. *Spirifera Verneuilii;* 6. *Palæoneilo maxima;* 7. *Sanguinolites rigida;* 8. *S. clavulus;* 9. *S. ventricosa?* 10. *Mytilarca Chemungensis;* 11. *Pteronites*, sp.; 12. *Pteronites*, sp.; 13. *Actinodesma recta;* 14. New form, undt.; 15. *Orthoceras crotalum?*

These were collected at one locality and in haste, the only object being to obtain a few specimens of the more common forms. Of the list, Nos. 6 and 13 are found in New York only in the Hamilton rocks, while No. 15 is very closely allied to a Hamilton species and may be identical with it; but respecting the other forms there is no doubt—they are Chemung. All of these forms occur also in the layers interstratified with those containing the fucoids. They are not stray specimens, such as might have been washed from the older into the newer rocks, for they are found in great abundance throughout the section and they are as well preserved as Chemung fossils usually are in New York. With these are immense quantities of fucoids, such as are characteristic of the Portage or lower Chemung in New York. But in the whole section there is not an *Anodonta*, not a fish-plate, not any fossil of any sort which can in any way be identified as belonging to the red Catskill of New York.

It is more than probable that the section represents only the lower portion of the Chemung and that not only the red Catskill, but also the upper portion of the Chemung is wanting in this part of Pennsylvania.*

What then has become of the great Catskill group? The upper or gray Catskill is represented, no doubt, by the Pocono or Vespertine sandstone, but the lower or red Catskill has disappeared. Nor is this disappearance at all strange. It is simply what might have been expected.

Professor H. D. Rogers, on pp. 141 and 142 of vol. i, of his Final Report, shows with what rapidity the Ponent or red Catskill thins out toward the northwest; that it is 5,000 to 6,000 feet thick in the southeast belt; 2,500 to 1,000 feet in the northwest belt; and 400 to 0 feet in the fifth belt; the diminution in each belt being distinct as one goes northwest or even west. No details are given respecting the variations of the group in a due west direction or towards the southwest, most probably because no possibility of tracing the group existed then any more than now. The presence of Ponent rocks is incidentally mentioned in notes upon the southern Alleghanies and the gaps through Chestnut and Laurel Ridges, but these observations were evidently regarded as too detached and too unimportant to be of value, since no reference is made to them in the general summary of the group given in vol. i of the Final Report.

All the evidence points in one direction. It is impossible by any stratigraphical work to make direct connection between the localities under consideration and those where the age of the rocks is settled beyond dispute; the lithological characters of the rocks in question are much like those of the Chemung, while the fossils, both animal and vegetable, are unquestionably of Chemung age. But one conclusion remains—the rocks are Chemung and, as already stated, probably represent only the lower Chemung; the great Catskill group has so far thinned out, that it is represented only by its upper or gray member, the Vespertine of Pennsylvania.

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* In the Proceedings of American Philosophical Society, vol. xvii, p. 270, it is stated that at 300 feet below the *Pittsburgh Coal bed*, or midway in the Lower Barren Series, certain Chemung fossils have been found. I have been advised that, while pleading the cause of the Chemung group in the gaps through Laurel and Chestnut Ridges, I would do well to explain how Chemung fossils happen to be present midway in the Coal measures.

No explanation is necessary further than to say that the species were wrongly identified. I have examined the specimens and have recognized the following species:—

Lophophyllum proliferum M'C., *Athyris subtilita* H., *Spirifer planoconvexus* Shum., *Orthis carbonaria* Swal., *Chonetes granulifera* Owen, *Productus pertenuis* Meek?, *Hemipronites crassus* M. and H., *Lima retifera* Shum., *Astartella vera* H.

These are usually thought to be quite characteristic of the Coal measures.

ART. LXII.—*Research on the Absolute Unit of Electrical Resistance;* by HENRY A. ROWLAND, Professor of Physics in the Johns Hopkins University, Baltimore, Md.

[Concluded from page 336.]

The Circle.—The circle whose constant we have called G'' and which was around the galvanometer whose constant was G, was a large wooden one containing a single coil of No. 22 wire.* To prevent warping, it was laid up out of small pieces of wood with the grain in the direction of the circumference, and was carefully turned with a minute groove near one edge in which the wire could just lie. It was about 5· cm. broad, 1·8 thick and 82·7 cm. diameter. As the room had no fire in it, the circle remained perfect throughout the experiment. The wire was straightened by stretching and measured before placing on the circle, which last was done with great care to prevent stretching; after the experiment it was measured and found exact to $\frac{1}{10}$ mm.

The circle was adjusted parallel and concentric with the coils of the galvanometer, but at a distance of 1·1 cm. to one side, in order to allow the glass tube with the suspending fiber to pass. The length of wire was 259·58 cm. which gives a mean radius of 41·31344 cm. These data give $G''=\cdot151925$. Preliminary results were also obtained by use of another circle.

Chronometer.—To obtain the time of vibration, a marine chronometer giving mean solar time was used. The rate was only half a second per day.

Wheatstone bridge.—To compare the resistance of the circuit with the arbitrary German silver standard, a bridge on Jenkin's plan, made by Elliott of London, was used. A Thomson galvanometer with a single battery cell gave the means of accurately adjusting the resistance, one division of the scale representing one part in fifty thousand.

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* In another part of my paper I have criticised the use of wooden circles for coils, but it is unobjectionable in the case of a single wire, especially when the needle is suspended near its center.

Thermometers.—Accurate thermometers graduated to half degrees were used for finding the temperature of the standard.

The arbitrary standard.—This was made of about seventy feet of German silver wire, mounted in the same way as the British Association Standard. Immediately after use, two copies, one in German silver and the other in platinum-silver alloy, were made. It had a resistance of about 35 ohms. The temperature was taken as 17° C.

To obtain the accurate resistance of this standard in ohms, I had two standards of 10 ohms and one of 1, 100, and 1,000 ohms. The 1-ohm, and one of the 10-ohm standards, were made by Elliott of London, and the others by Messrs. Warden, Muirhead and Clark of the same place. But on careful comparison I found that Warden, Muirhead and Clark's 10-ohm standard was 1·00171 times that of Messrs. Elliott Bros. On stating these facts to the two firms I met no response from the first firm, but the second kindly undertook to make me a standard which should be true by the standards in charge of Professor Maxwell at Cambridge.* At present I give the result of the comparison with these standards, as well as some others, and also with a set of resistance coils by Messrs. Elliott Bros.

Commutators.—No commutators except those having mercury connections were used, and those in the circuit whose resistance was determined were so constructed as to offer no appreciable resistance. The commutator by which the main current was reversed, could be operated in a fraction of a second, so as to cause no delay in the reversal.

Connecting wires.—These were of No. 22 or No. 16 wire and were all carefully twisted together. The insulation was tested and found to be excellent.

Inductor for damping.—This has already been described in my first paper on "Magnetic Permeability," and merely consisted of a small horse-shoe magnet with a sliding coil, which was introduced into the secondary circuit. By moving it back and forth, the induced current could be used to stop the vibrations of the needle and make it stationary at the zero point. This is necessary in the method where the first throw of the galvanometer needle constitutes the observation, but in the method of recoil it is not necessary to use it very often. I prefer the method of the first throw as a general rule, but I have used both methods.

This method of damping will be found much more efficient than that of the damping magnet as taught by Weber, and after practice a single movement will often bring the needle exactly to rest at the zero point.

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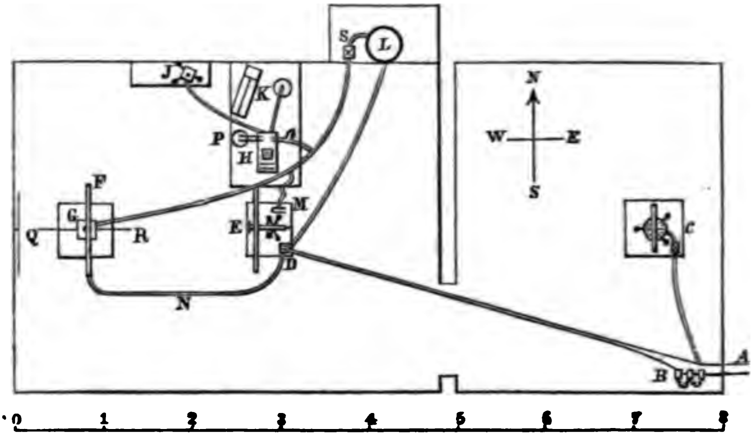
* As this is nearly a year since, and as I cannot tell when the standard will arrive, I now publish the results as so far obtained, hoping to make a more exact comparison in future.

Arrangement of apparatus.—Two rooms on the ground floor of a small building near the University were set aside for the experiment, making a space 8 m. long by 3·7 m. wide. The plan of the arrangement is seen at fig. 1. The current from

1.



the battery, in the University, entered at A, the battery being eighteen one-gallon' cells of a chromate battery, arranged two abreast and eight for tension. The resistance of the circuit was about 20 ohms, and of the whole battery about ½ ohm, thus insuring a reasonably constant current.

At B some resistance could be inserted by withdrawing plugs so as to vary the current.

At C is the tangent galvanometer with commutator on a brick pier. The nearness of the commutator produces no error, seeing that we only wish to determine the ratio of two currents. The effect of currents in the commutator was, however, vanishingly small in any case.

At D is the principal commutator which reversed the current in the induction coils, L, or in the circle, F, when it was in the circuit.

The secondary circuit included the induction coil, L, the damping inductor, M, and the galvanometer G.

At H was the Jenkin's bridge, with standard at P, in a beaker of water, and a Thomson galvanometer at J K. The secondary circuit could be joined to the bridge by raising a U-shaped piece of wire out of the mercury cups.

The telescope and scale, E, were on a heavy wooden table, and the two galvanometers on brick piers with marble tops.

A row of gas-burners at Q illuminated the silvered scale in the most perfect manner.

Adjustments and tests.—The circle, F, must be parallel to coils of galvanometer, G. The circle and coils of galvanometer were first adjusted with their planes vertical and then adjusted in azimuth by measurement from the end of the bar, R, to the sides of the circle, F. The adjustment was always within 30′, which would only cause an error of one part in 25000.

The needle must hang in the magnetic meridian by a fiber without torsion, and the coils must be parallel to it. These adjustments were carefully made, but, as has been shown, the error from this source is compensated.

The needle must hang in the center of the galvanometer coils and on the axis of the circle. The error from this source is vanishingly small.

The scale must be perpendicular to the line joining the zero point and the galvanometer needle, it must be level and not too much below the galvanometer needle. All errors from this source are partially or entirely compensated by the method of experiment.

The induction coils, L, must be horizontal, and at the same level as the two galvanometers, so as not to produce any magnetic action on them. The error from this source is exactly compensated by this method of experiment, but could never amount to more than 1 part in 2000.

The tangent galvanometer should have the plane of its coils in the magnetic meridian, but all errors are compensated.

The connecting wires must be so twisted together and arranged as to produce no magnetic action, but tests were made in all cases where the error was not compensated, and found to be practically zero. The insulation of all coils, wires and commutators was carefully tested.

Method of Experiment.—As has been stated before, the method generally used was that of the first throw of the needle, though the method of recoil was also used. For the successful use of the first method a quickly vibrating needle and the damping inductor are indispensable, seeing that with a slow moving needle we can never be certain of its being at rest. By this method it is not necessary to have the needle at rest at the zero point, but, if it vibrates in an arc of only a millimeter or two, we have only to wait till it comes to rest at its point of greatest elongation on either side of the zero point and then reverse the commutator. The error by this method is in the direction of making the throw greater in proportion of the cosine of the phase to unity. The smallest throw used was 100 mm. Hence, if the needle vibrated through a total arc of 2 mm., the error would be 1 in 17,000. In reality the needle was always brought to rest much more nearly than this.

The method of recoil was used once with the needle vibrating in 7·8 seconds, but the time of vibration was too short and another needle was constructed vibrating in 11·5 seconds, which was a sufficiently long period to be used successfully after practice.

There seems to be no error introduced by the time taken to reverse the commutator in the method of recoil, seeing that the breaking of the current stops the needle and the making starts it in the opposite direction. As the time was only a fraction of a second the error is minute in any case.

While the current is broken in the reversal, the battery may recuperate a little and there is also some action from the extra current, but there seems to be no doubt that long before the four or six seconds which the needle takes to reach its greatest elongation everything has again settled to its normal condition and the current resumes its original strength. Hence the error from these sources may be considered as vanishingly small.

Some experiments were made by simply breaking the current and they gave the same result as by reversal.

The following is the order of observations corresponding to each experiment.

1st. The time of vibration of needle was observed.

2d. The current was passed around the circle, F, so as to observe β and α. Simultaneous readings were taken at the two galvanometers. The commutator at the tangent galvanometer was then reversed and readings again taken. After that the commutator to the circle was reversed and the operation repeated. This gave four readings for the circle and eight for the tangent galvanometer, as both ends of the needle were read. In some cases these were increased to six and twelve respectively. This operation was repeated three times with currents of different strengths, constituting three observations each of α and β. To eliminate any action due to the induction coils, they were sometimes connected in one way and sometimes in the opposite way.

3d. The resistance of the circuit was adjusted equal to the arbitrary standard.

4th. The circle, F, was thrown out the circuit and the observations of θ and δ begun. Two throws, δ, one on either side of zero were observed and one reading of θ taken. The commutators at s and C were then reversed, and the operation repeated. This whole operation was then repeated with currents of three different strengths. The position of the two induction coils was now reversed and observations again made with the three currents. The resistance was now compared with the standard, the difference noted, and the resistance again adjusted. The observations were completed by turning the induction coils into the two other positions which they could occupy with respect to each other, followed by another comparison of resistance with standard.

5th. Observations of α and β were again made as before.

6th. The time of vibration was again determined.

The observations as here explained furnished data for three computations of the resistance of the circuit, one with each of the three currents. In each of these three computations, α was the mean of 16 readings, β of 8 or sometimes 12, θ of 16 and δ of 16. In using the method of recoil nearly the same order was observed.

The time of vibration was determined by allowing the needle to vibrate for about ten seconds and making ten observations of transits before and after that period. During the experiment, I usually observed at the telescope and Mr. Jacques at the tangent galvanometer.

The methods of obtaining the corrections require no explanation.

Results.

The constant corrections are as follows for the first needle.

$$a= -\tfrac{1}{2}\lambda+\tfrac{5}{12}\lambda^2= -{\cdot}00711$$
$$b= -\tfrac{1}{2}t= -{\cdot}00020$$
$$c= -{\cdot}000006$$
$$d= +{\cdot}000074 \textit{ at } 20{\cdot}^\circ \text{ C.}$$
$$e=\frac{1}{128_n\lambda}\left(c_1^2-c_n^2\right)= +{\cdot}00003$$
$$f= +{\cdot}00003$$
$$a+b+c+d+e+f= -{\cdot}00718.$$

For method of recoil it becomes $-{\cdot}00016$.

Hence for A and B, log K=11·4536030

" " A and C, log K=11·2852033

" " B and C, log K=11·1886619

For method of recoil using A and B, log K=11·4566630.

For second needle and method of recoil,

$$a= -\tfrac{1}{2}\left(\frac{\lambda}{\pi}\right)^2= -{\cdot}000050$$
$$b= -\tfrac{1}{2}t= -{\cdot}00025$$
$$c= -{\cdot}000006$$
$$d= +{\cdot}000074$$
$$e= +{\cdot}00003$$
$$f= +{\cdot}00003$$
$$a+b+c+d+e+f= -{\cdot}00017$$

For A and B, log K=11·4566587

" A and C, log K=11·2882590

" B and C, log K=11·1917176

The distance of the mirror from the scale varied between 192·3 and 193·5 cm.

Table of Results.

Induction coils.	Method.	T	Tem. of Standard.	β	δ	α	θ	Corrections. A +·000	Corrections. B —·00	Corrections. C +·00	Corrections. D —·000	Resistance in earth quad. sec.	Mean resistance in earth quad. sec.	Error.	Per-centage error.
A & C	0	7·800	18°·7	220·17	170·29	40° 12′·44	42° 32′·56	19	075	036	22	34·672	34·748	+·029	+·083
	0	"	"	185·61	141·28	35 29·06	37 22·94	"	"	029	"	34·784			
	0	"	"	173·83	131·40	33 43·56	35 28·01	"	"	025	"	34·787			
B & C	0	7·800	20·3	222·20	137·07	40 27·81	42 49·81	21	145	036	74	34·760	34·714	—·005	—·014
	0	"	"	186·66	113·70	35 41·94	37 33·69	"	"	029	"	34·680			
	0	"	"	174·64	105·71	33 55·87	35 35·94	"	"	025	"	34·702			
A & C	0	7·806	21·0	219 66	168·16	40 7·56	42 18·06	22	176	035	44	34·762	34·773	+·054	+·155
	0	"	"	184·92	139·65	35 26·37	37 7·87	"	"	025	"	34·725			
	0	"	"	173·20	129·84	33 40·75	35 13·06	"	"	024	"	34·831			
A & B	0	7·822	24·7	208·65	235·98	38 41·87	40 56·13	29	334	034	44	34·700	34·716	—·003	—·008
	R	"	"	176·31	119·20	34 10·50	40 57·17	"	"	102	"	34·733			
		"	"	165·20	117·48	32 30·56		"	"	126	"				
B & C	0	7·826	25·4	211·15	129·29	39 3·87	41 19·19	82	870	034	44	34·730	34·718	—·001	—·003
	0	"	"	178·01	107·48	34 27·31	36 13·25	"	"	027	"	34·723			
	0	"	"	166·96	100·27	32 46·12	34 19·87	"	"	025	"	34·700			
A & B	0	7·828	25·9	210·53	237·74	39 1·69	41 11·62	82	892	034	43	34·665	34·685	—·034	—·098
	0	"	"	177·71	197·72	34 24·00	36 7·50	"	"	025	"	34·704			
	0	"	"	166·32	184·35	32 40·00	34 14·56	"	"	024	"	34·686			
A & B	R	11·488	19·8	219·62	85·407	40 0·16	42 14·65	21	123	039	88	34·701	34·688	—·031	—·089
	R	"	"	185·15	84·651	35 20·82		"	"	108	"	34·667			
	R	"	"	173·64		33 37·31		"	"	129	"	34·697			
A & C	R	11·487	20·6	216·80	56·630	39 37·43	41 35·22	22	158	030	22	34·724	34·712	—·007	—·020
	R	"	"	183·38	56·059	35 2·81		"	"	100	"	34·725			
	R	"	"	172·92		33 31·25		"	"	121	"	34·688			

Mean, 34·7192 ± ·0070

Should we reject the quantity 34·831 in the third experiment so as to make the mean result of that experiment 34·744 instead of 34·773, we should obtain as a mean result of the whole

$$34{\cdot}7156 \pm {\cdot}0053,$$

which has a less probable error than when the above observation is retained. The number of plus and minus errors are also more nearly equal and the greatest difference from the mean 1 part in 1100. However the two results do not differ more than 1 part in 10,000.

We shall take

$$R = 34{\cdot}719 \pm {\cdot}007 \ \frac{\text{earth quad.}}{\text{second.}} \text{ at } 17^{\circ}\text{ C.}$$

as the final result.

Discussion.

On glancing over the table we see that the number of negative errors greatly exceed the number of positive, but, if we take only the four errors which are greater than 1 part in 5,000, we shall find two of them negative and two positive.

Combining the results with the different coils we have

A and B..............34·696 ± ·005
A and C..............34·744 ± ·011
B and C..............34·716 ± ·007

Had we no other results to go by, we might suppose that the value of M might not have been found as exactly for these coils as we have supposed them to be. But if we include the preliminary results rejected on account of the imperfect circle used, we shall find

A and B..............34·704 ± ·006
A and C..............34·718 ± ·017
B and C..............34·758 ± ·016

which has the greatest error in an entirely different place.

From the first series the probable error of each determination of M is 1 in about 2,000. But as this includes the experimental errors which are about equal to $\pm\frac{1}{4000}$, the real probable error of M must be about 1 part in 2,500. The number of observations is however too small for an exact estimate of the probable errors.

Taking the results with currents of different strengths, we find

For strongest current	34·716
" medium "	34·715
" weakest "	34·727

which are almost perfectly accordant. Taking the results from the method of recoil and the ordinary method, we find

For ordinary method	34·726 ± ·010
" method of recoil	34·705 ± ·006

If the probable error is subtracted from the first and added to the second they will very nearly equal each other. Hence the difference is probably accidental. Indeed, by the combination of the results it does not seem possible to find any constant source of error, and therefore the errors should be eliminated by the combination of the results.

In the final result

$$R = 34{\cdot}7192 \pm {\cdot}0070$$

the probable error, ±·0070, includes all errors except the ratio of G to G″. We may estimate the probable error of G at $\pm\frac{1}{3000}$ and of G″ at $\pm\frac{1}{3000}$.

Hence the final probable error of R, including all variables, is $\pm\frac{1}{2300}$, or ±·04 per cent,

$$\text{or } R = 34{\cdot}719 \pm {\cdot}015.$$

The probable error of the British Association determination was ±·08 per cent, not including the probable error of the constants; and of Kohlrausch's determination ±·33 per cent, including constant errors.

Comparison with the Ohm.

The difficulty in obtaining proper standards for comparison has been explained above and I shall have to wait until the arrival of the new standard before making the exact comparison. At present I give the following results, which seem to warrant the rejection of Messrs. Elliott Bros'. 10-ohm standard and to make that of Messrs. Warden, Muirhead and Clark correct. I shall designate the coils by the letter of the firm and by the number of ohms. Experiment gave the following results:

W (10)=1·00171 × E (10), experiment of June 8, 1877.
W (10)=1·00166 × E (10), " " Feb. 23, 1878.

W (1,000) : W (100) : : W (10) : ·999876 E (1), experiment of February 23, 1878.

Now the greatest source of error in making coils is in passing from the unit to the higher numbers. As the reproduction of single units is a very simple process the single ohm is without much doubt correct, and as the above proportion is correct within one part in 8,000 of what it should be, it seems to point to the great exactness of the standards then used, seeing that the exactness of the proportion could hardly have been accidental. It is also to be noted that Messrs. Warden, Muirhead & Clark's 10-ohm standard agreed more exactly with a set of coils by Messrs. Elliott Bros. than their own unit E (10).

The resistance of my coil as derived from the different standards is as follows:

From Elliott Bros. resistance coils	34·979	ohms.
" " " 10-ohm standard	35·083	"
" W., M. & C.'s " "	35·024	"
" W., M. & C.'s 100-ohm "	35·035	"

These give for my determination the values of the ohm as follows:

From Elliott Bros. resistance coils,	·99257	$\frac{\text{earth quad.}}{\text{sec.}}$
" " " 10-ohm standard,	·98963	"
" W., M. & C.'s " "	·99129	"
" W., M. & C.'s 100-ohm "	·99098	"

For the reasons given above I accept the mean of the last two results as the value of the ohm.

To preserve my standard I have made two extra copies of it, the one in German silver and the other in platinum silver alloy. The comparisons are given below. No. 1 is in German silver and the other in platinum silver alloy. The temperature is 17·° C.

No. I................	1·00034	June, 1877.
No. I................	1·00029	Feb., 1878.
No. II...............	·99630	June, 1877.
No. II...............	·99932	Feb., 1878.

These are the values of the copies in terms of the original standard whose resistance is 34·719 $\frac{\text{earth quad.}}{\text{sec.}}$

From these results it would seem that the German silver of which the standard and No. I were composed was perfectly constant in resistance. The wire has been in my possession for several years and seems to have reached its constant state.

The final result of the experiment is

$$1 \text{ ohm} = \cdot 9911 \frac{\text{earth quad.}}{\text{sec.}}$$

ART. LXIII.—*The Ancient Outlet of Great Salt Lake;* by A. C. PEALE.

IN this Journal for April, 1878, pp. 256–259, is an article entitled "The Ancient Outlet of Great Salt Lake: a letter to the editors by G. K. Gilbert." In this article Mr. Gilbert states that "previous to 1876 the outlet was not discovered, or if discovered, its position was not announced," and that "in the summer of that year" he "had the great pleasure of finding it in Idaho, at the north end of Cache Valley, the locality being known as Red Rock Pass." He says also that the announcement was made by him "without reservation in a communication to the Philosophical Society of Washington,"* and that the announcement was also made for him "in the same unequivocal manner" "in the Smithsonian Report for 1876" (p. 61), and "in Baird's Annual of Scientific Discovery for 1876"* (p. 206), and that "there seemed to be no occasion for further publication until the matter should receive its full discussion in the Reports of the Survey of which Professor Powell has charge," but owing to a statement † in this Journal for January, 1878, p. 65, "it seems proper" to him "to defend" his "*positive* assertions by setting forth the facts which appear" to him "to place the existence and position of the ancient outlet *beyond question*."‡

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* At the 116th meeting of the Society, January 13, 1877, "Mr. G. K. Gilbert made a communication on Lake Bonneville, the great fossil lake of Utah. He described an ancient outlet of the lake at Red Rock Pass near the town of

As Red Rock Pass, the point of Mr. Gilbert's discovery (?), is within the area assigned during the season of 1877 to Mr. Gannett's division of the United States Geological Survey of the Territories, with which I was connected as geologist, it seems proper that I should call attention to several errors in Mr. Gilbert's statements.

In the first place, *his so-called discovery is not a discovery on his part.* The fact that Red Rock Pass was an outlet for the lake that once filled the Salt Lake Basin and adjoining valleys §

Oxford, Idaho, by which its waters were discharged into Snake River. During and since the desiccation of the lake, the land which it covered has been tilted to the northward in common with the region of the Laurentian lakes and the eastern and western seaboard." (Bulletin of the Philosophical Society of Washington for 1877, p. 103.)

* The "unequivocal" announcement referred to by Mr. Gilbert, is stated in exactly the same words in both publications, and is made in the following rather *vague* manner. "Before commencing the main work of the season, Mr. Gilbert made an excursion in search of the outlet of Lake Bonneville, the great fossil lake of Utah." * * * "The search for the point of outlet was successful, and it was found at the *north end of Cache Valley, a few miles beyond the boundary of Utah, in the Territory of Idaho.* (Annual Report of the Board of Regents of the Smithsonian Institution for 1876. Washington, 1877, p. 61) and (Annual Record of Science and Industry for 1876, edited by Spencer F. Baird. New York, 1877, p. 260.)

† The statement referred to is the following: "It is believed that the explorations of the survey under the direction of Dr. Hayden, the past season, have determined the probable ancient outlet of the great lake that once filled the Salt Lake Basin." (This Journal, vol. xv, Jan., 1878, p. 65.)

‡ The italics in this paragraph are my own.

§ The following extracts from the Report of the Survey for 1870, written by Dr. Hayden, show that as early as that time the extent of the great inland basin and its real conditions were truly appreciated by him.

"Let us for a moment take a bird's-eye view of the great inland basin of which Salt Lake Valley forms only a part. We shall find that what is termed the Great Basin of the West comprises the vast area inclosed by the Wasatch Mountains on the east, and the Sierra Nevada on the west, the crest or water divide of the Columbia on the north, and that of the Colorado on the south. We shall also observe that this great region has no visible outlet; that it is composed of a multitude of smaller basins or valleys, each of which has its little lakes, springs and water-courses, their surplus water either evaporating or sinking beneath the surface. If we examine the elevations in this region, we observe a wonderful uniformity in the surface of the valleys, and find that none of them are much above the level of the waters of Great Salt Lake." (p. 172.)

"I infer that a vast fresh-water lake once occupied all this immense basin; that the smaller ranges of mountains were scattered over it as isolated islands, was not only recognized, but well known five years prior to Mr. Gilbert's supposed discovery. On page 202 of the Annual Report of the Survey for 1872[*] is the following statement by Professor F. H. Bradley: "The level of the divide between the head of Marsh Creek and the Bear River drainage, at Red Rock Pass, as ascertained by the party of 1871, indicates that this was probably another point of outflow;" and on the following page (203) the following sentence in relation to the terraces in Marsh Creek Valley, which extend northward from Red Rock Pass: "They are on too large a scale, and the valley is too wide, to have resulted from merely the drainage of the small area of mountains about the head of the stream; and I am strongly of the opinion that this must have been at one time the channel for a large outflow from the Great Basin."

It seems to me that this places the discovery where it belongs beyond question.

It appears also that Mr. Gilbert ignores some of his own statements. In his report to Lieutenant Wheeler,[†] he says that Professor O. C. Marsh informed him that he had discovered on the northern shore of the lake an outlet leading to Snake River, and in a foot note on the same page says, "Professor Frank H. Bradley mentions four points of possible outflow from the northeast margins.—(*United States Geological Survey of the Territories*, 1872, p. 202.)"

In the second place, *Red Rock Pass was not the outlet of Lake Bonneville.*

Lake Bonneville extended over the whole of Marsh Creek Valley and its outlet was more than forty miles farther north than Mr. Gilbert ever went. Red Rock Pass was only a point of stricture in the lake.

In his article (p. 257), Mr. Gilbert says: "In Marsh Creek Valley the eye seeks in vain for the familiar shore lines of the Salt Lake Basin, and the conclusion is irresistible that here the ancient lake outflowed." This sentence implies that he went into Marsh Creek Valley. Had he done so or had he even ascended one of the numerous points that command the view

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their summits projecting above the surface; that the waters have gradually and slowly passed away by evaporation, and the terraces are left to reveal certain oscillations of level and the steps of progress toward the present order of things; and that the briny waters have concentrated in those lake basins, which have no outlet." (p. 170.)

This was written before Mr. Gilbert began his western investigations.

Dr. Hayden also first indicated Cache Valley as one of the bays of the ancient lake. (Report United States Geological Survey for 1871, p. 19.)

[*] Sixth Annual Report of the United States Geological Survey of the Territories for 1872, Washington, 1873.

[†] Report upon Geographical and Geological Explorations west of the 100th meridian, in charge of First Lieutenant Geo. M. Wheeler. Vol. iii, Geology. Washington, 1875, p. 91.

of both valleys (Cache, and Marsh Creek) the relation of the two must have been apparent.

The "gentle alluvial slopes" mentioned by Mr. Gilbert (on page 257) as being "divided for several miles by a steep-sided, flat-bottomed, trench-like passage a thousand feet broad, and descending northward from the divide" are *white sandstones* similar to those in the bottoms of Cache and other valleys of the Salt Lake Basin. The following elevations on the terraces in Marsh Creek Valley were obtained by barometrical observations.*

Two miles north of Red Rock Pass on the east side of the valley, 5,137 feet.

Six miles west of Red Rock Pass on the edge of Marsh Valley, 5,053 feet.

Twenty-six miles from Red Rock Pass on the west side of the valley, 5,117 feet.

The elevation of the Bonneville beach is 5,185·7 feet † and it is evident that the Red Rock Gap (the walls of which do not exceed the elevation of 5,000 feet) could not have been a barrier to Lake Bonneville. The conclusion is therefore irresistible that the result of Mr. Gilbert's four or five years' search is a mistake.

In the third place *Red Rock Pass was an outlet, but it was the outlet probably when the lake was at the level indicated by the Provo Beach.* When the barrier at the northern end of the Bonneville Lake was removed, that portion of the lake occupying Marsh Creek Valley was completely drained, and Red Rock Gap became the barrier of the lake that remained. Then it was that the course of Marsh Creek began to be outlined, and the lowering of the lake was doubtless comparatively rapid until the level of the Provo Lake was reached. The line of the Provo Beach indicates a period of comparative permanence, but when the pass became lower than the lake, of course the lake was drained. The elevation of the pass as obtained by railroad level is 4,792 feet, and the Provo Beach, according to Mr. Gilbert, is about 365 feet below the Bonneville Beach,‡ which would give an elevation of 4,820·7 feet for the former.

In the fourth place I wish to call attention to two more of Mr. Gilbert's statements. On page 258 he says, "In Dr. Hayden's Preliminary Report of the field work of his survey for the season of 1877, noticed on page 56 of the current volume of this Journal, there is no mention of the observations at Red Rock Pass, but *the omission appears to have been accidental*, &c." The portion of this statement that I have italicised is a gratuitous assumption. The omission was *not* accidental. I did not believe that the outlet was at Red Rock Gap, and in the Preliminary Report (page 7), I made the following statement: "The lower valley of the Portneuf is interesting from the fact that it is the probable ancient outlet of the great lake that once filled the Salt Lake Basin."*

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* I am indebted to Mr. Henry Gannett for all the elevations I use and for other valuable information obtained by him while in the field.

† This elevation is obtained by adding 967·7 feet (the height of the Bonneville Beach above Salt Lake, given by Mr. Gilbert in the Report of Geographical and Geological Explorations west of the 100th meridian, vol. iii, p. 92) to 4,218 feet, the elevation of Salt Lake, which was obtained from the chief engineer of the Union Pacific Railroad.

‡ This Journal, vol. xv, April, 1878. p. 258.

Mr. Gilbert also hopes that I "will not advocate in" my "report the idea that the divide between the Malade and Marsh Creek was one of the old outlets of the ancient Salt Lake when its waters were at the highest level."

Had I been writing a final report on the subject I would perhaps have used the word overflow instead of outlet. It seems, however, scarcely necessary to refer to this point, as Mr. Gilbert himself acknowledges that he "did not ascend to the summit, although" he "had undertaken last summer to examine every divide between the Columbia and Salt Lake Basins, that might have afforded passage to the water."†

In all his investigations he seems never to have noted any evidences of a lake having a higher level than his Lake Bonneville. Such evidences, however, do exist. On both sides of the Portneuf where it comes into Marsh Creek Valley an upper terrace is seen, and in 1872 Professor F. H. Bradley also readily identified an upper terrace in Marsh Creek Valley at the level of about 1,000 feet above the stream.‡ In Gentile Valley and in Cache Valley also, traces of this upper terrace exist. The elevation of the Malade divide is 5,650 feet and the level of this higher lake as indicated by the elevation of the terraces must have been between 5,500 and 5,800 feet, so that when the waters were at the highest level there was no impossibility of communication over the Malade divide.

I have read and re-read Professor Bradley's Report and I see no warrant for the statement that he made the "astonishing suggestion that four outflowing streams might have *coexisted.*" (The italics are my own.)

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* Preliminary Report of the field work of the United States Geological and Geographical Survey of the Territories for the season of 1877. Washington, 1877, p. 7.

† This Journal, vol. xv, April. 1878, p. 258. Mr. Gilbert appears to take it for granted that the point of outlet must necessarily be found at one of the existing divides between the Great Basin and the Columbia.

The railroad profile from the bluffs on Bear River in Cache Valley to Red Rock Pass (a distance of fifteen miles) shows a difference of elevation of only eleven feet, and the descent of Marsh Creek from Red Rock Pass to a point twenty-six miles farther north averages only 1·07 feet per mile. This indicates that the divide is not well marked and that previous to the erosion that followed the draining of Lake Bonneville it may easily have been much farther north. At present the divide for several miles is a swamp.

‡ Sixth Annual Report of the United States Geological Survey, for 1872. Washington, 1873, p. 203.

In conclusion I wish to state that this paper is based on the combined observations of Mr. Henry Gannett and myself, made during the progress of our regular field work without any special reference to finding an outlet of the ancient Salt Lake. Mr. Gilbert has spent portions of at least two seasons in the study of this special subject in the northern portion of the basin, and it is evident that his investigations are still unsatisfactory.

ART. LXIV.—*Note on the Ferment-theory of Nitrification;* by F. H. STORER, Harvard University.

THE results of the following experiments bear so immediately upon the recent observations of Schlösing* and Warington,† noticed in the April number of this Journal, that I am led to publish them by themselves, out of their legitimate connection with other experiments upon which I have been for some time engaged. My experiments were made for the purpose of testing the action of certain oxidizing agents on ammonium compounds, and they are, perhaps, not the less interesting as a contribution to the ferment-theory of nitrification, inasmuch as they were undertaken without any reference to the observations of Schlösing, and not at all with the view of supporting or even of testing his experiment.

For my first trial, eleven common glass bottles of greenish tint, each of 500 cc. capacity, and fitted with corks carrying inlet and outlet tubes, were charged as follows: No. 1 with distilled water that had been carefully freed‡ from nitrates, nitrites, and ammonium compounds; No. 2 with a solution of ammonium chloride, containing $\frac{3}{10}$ milligram of NH_3 to the centimeter; No. 3 same as No. 2, with the addition of a quantity of ferric hydrate; No. 4 same as No. 2, with the addition of ferrous hydrate; No. 5 same as No. 3, with the addition of charcoal made from white sugar; No. 6 ammonium chloride, black oxide of manganese and sugar charcoal; No. 7 leached peat and pure water; No. 8 leached peat and ammonium chloride; No. 9 leached peat and ferric hydrate; No. 10 leached peat, ferric hydrate, and ammonium chloride; No. 11 pure water.

Pure water was used in each case to dissolve or suspend the chemicals, and the absence of nitrates and nitrites was proved by testing each of the solutions and mixtures with iodo-starch, at the beginning of the experiment.

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* Comptes Rendus, lxxxiv, 301.

† Journal of London Chem Soc., 1878, i, 44.

‡ In the manner described in the note on page 182 of vol. xii of this Journal.

The bottles were about half filled with liquid, i. e., each of them contained about 250 cc. of the solution or mixture allotted to it.

The "leached peat" was prepared from some bog-meadow mud from the Bussey farm, which had been kept in barrels in a dry store-room for three or four years. This thoroughly air-dried substance was percolated with pure water until the filtrate gave no reaction for nitrites or nitrates.

The ferric and ferrous hydrates were used in the recently precipitated condition; they were made from the corresponding chlorides by precipitating with ammonia-water, in the cold.

The bottles were connected with one another, in the order indicated, with short pieces of caoutchouc tubing in such manner that by aspirating at No. 1 air could be made to bubble through the water in each member of the series. The corks of the bottles and the caoutchouc connectors were covered with shellac in alcohol so that the entire apparatus was completely air-tight. That is to say, none of the solutions had the least connection with the external air, excepting as it was purposely admitted at the inlet-tube. To purify the air that was drawn into the apparatus, it was made to pass through a long tube loosely filled with clean cotton wool; through two sets of Liebig bulb-tubes filled with a tolerably strong solution of yellow prussiate of potash, to remove ozone; through two sets of similar bulbs charged with potash-lye to remove nitrates and nitrites; through a dry bottle, to catch liquid drawn forward from the potash bulbs; and through a large drying tube charged for two-thirds its length with calcium chloride and one-third with soda-lime.

By means of a Bunsen filter-pump a rapid current of air from out-of doors was drawn incessantly night and day, through the series of bottles, for a fortnight, in September, 1877, in the direction from No. 11 to No. 1; at the end of which time the contents of each bottle were tested for nitrites and nitrates, by boiling with cadmium, distilling with acetic acid, and mixing iodo-zinc starch and acid with the distillate. 100 cc. of liquid were taken for each of the tests, and fifteen minutes were allowed in which to watch for the appearance of the blue coloration. Nothing could be more distinct and decisive than the results of these tests. The solutions from bottles Nos. 8, 9 and 10 namely gave immediate and strong reactions for the nitrogen oxides, while the contents of Nos. 1, 2, 3, 4, 5, 6, and 11 gave no reaction whatsoever. No. 7 gave a reaction, but not a very strong one. The reaction seemed to be strongest in the contents of No. 10. It appeared from these results that there had been formation either of a nitrate or nitrite in each of the bottles which contained humus, and no such formation in either of the other bottles: but the thought suggested itself, that the small amount of nitrogen oxides found in No. 7 may perhaps have been dragged over mechanically from No. 8 by the current of air.

A second series of tests was made upon Nos. 8, 9 and 10, to see which of them gave the weakest reaction, 25 cc. of liquid being taken from each bottle and diluted with pure water to the volume of 100 cc. before applying the test. It appeared again that No. 10 gave the strongest reaction and that No. 8 gave the weakest. Roughly estimated, the strength of the reactions from jars Nos. 10, 9 and 8 were to one another as 5 : 2 : 1.

It may here be said that humus was employed in these experiments for the sake of testing the old observation of Millon,* who noticed that ammonium salts are changed to nitrates when in contact with oxidizing humus, and who argued that the chemical action originated by the coming together of humus and oxygen, was communicated to the ammonium compound. In his own words: "The oxidation of the humic acid is the cause of the oxidation of the ammonia."

In so far as the foregoing experiments go, we have manifestly a striking confirmation of the accuracy of Millon's work. But on repeating the experiments with the difference that the bog-earth was henceforth exhausted with hot, strong muriatic acid, before proceeding to wash it with water, no such results as the foregoing were obtained. For example, in a second series of trials, arranged like the first, the contents of bottles Nos. 1 to 6 were identically the same as before, only that the amount of liquid was now 150 cc. instead of 250 cc. No. 7 contained the purified peat mixed with water; No. 8 was the same as 7 plus a quantity of gypsum; No. 9 was the same as 8 plus some ammonium chloride; No. 10 contained water alone; No. 11 purified peat plus ammonium chloride and ferric hydrate; No. 12 well-washed cotton rags plus ammonium chloride and ferric hydrate; No. 13 purified peat plus ammonium chloride; No. 14 pure water; No. 15 pure water, the same namely, which had already done service (as No. 11), in the first series of experiments.

The weather (October 13, 1877), being too cold to permit the keeping open of a window, the current of air, purified as before, was drawn from the cold-air-box of the hot-air furnace used for warming the laboratory. The current of air passed this time in the direction from No. 1 to No. 15; it was maintained constantly during ten days, and the contents of the bottles were then tested as before for nitrites and nitrates. But no reaction was obtained in either instance, with the exception of No. 12 (cotton rags, etc.), which gave a faint coloration of a not very satisfactory character. After the application of the test those of the bottles which still contained a sufficiency of liquid were re-attached to the aspirator and air was drawn through them continually during another week, when the test for nitrites and nitrates was again applied. But in no case was there any reaction, with the exception of bottle No. 8 whose contents gave a faint coloration.

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* Kopp and Will's Jahresbericht der Chemie, 1860, xiii, 101 and 1864, xvii, 158.

Inasmuch as warm, and, at times, hot summer weather had prevailed during the term of the first series of experiments, while the out-door air was decidedly cool and that of the laboratory by no means very warm during the time allotted to the second series, a third set of trials was undertaken in December, 1877, in which the contents of the bottles were heated artificially. That is to say, the bottles were immersed in a large water-bath, which was heated to from 70° to 80° C. during the progress of the experiment.

In this third series, there were eight bottles containing the purified peat, first by itself; then admixed with gypsum; with gypsum and ammonium chloride; and with ammonium chloride and ferric hydrate. One bottle contained washed cotton rags, ammonium chloride, and ferric hydrate as before. The water-bath was heated during working hours for a fortnight, perhaps a hundred hours in all, and during this time air was drawn from the furnace-box and purified as before. The contents of the bottles were then tested for nitrites and nitrates, but no reaction was obtained in either case. To make sure that the absence of the reaction was not due to any interference caused by the presence of the peat or the chemicals, a fresh portion of liquid was taken from each of the bottles, enough nitrate of potash to amount to 0·001 gram of N_2O_5 was added, and the test for nitrites and nitrates was applied in the usual way: reactions were now obtained immediately in every instance.

In the light of the facts observed by Schlösing, the natural inference from the results of these experiments is that the formation of nitrates or nitrites in bottles Nos. 7 to 10 of the first series of experiments was due to the presence of living organisms which the peat had harbored, and that the absence of nitrification in the other series of experiments is to be attributed to the destruction of the ferment-germs by the hot acid with which the peat employed in these experiments had been treated. It is to be observed, moreover, that the formation of nitrogen oxides in the bottles Nos. 7 to 10 is in nowise out of accord with the important fact observed by Warington, that darkness* is essential to the action of the nitrifying germs, for although my bottles were not shielded from diffused daylight, the mixtures of peat and water which they contained were practically dark-colored muds, not ill-fitted to shelter the germs from the light.

It is possible of course that the colder weather which prevailed during the later trials may have had an influence upon their results; but if this be so, the fact must be counted as an additional argument in favor of the ferment-theory. Moreover, it may fairly be inferred that if mere oxidation be all that is needed to induce nitrification, the heating of the liquids by day in the third series of experiments would have been sufficient; though both the strong heat and the cooling of the bottles by night would have tended to prevent the growth of living organisms.

In the interest of the ferment-theory, it would have been well to control the negative results above given by trials with mixtures of the purified peat and carbonate of lime, for peat which has been treated with muriatic acid has always a slight acid reaction, due I suppose to free humic acid, no matter how thoroughly it may have been washed with water, and it is to be supposed that this acid peat, devoid withal of phosphatic and other saline matters, is not favorable for the growth of the ferment. But as was said before, my experiments were made to test the oxidizing action of certain chemicals, not to cultivate living organisms.

It may be added that I have not as yet found any evidence that solutions of ammonium compounds can be oxidized to nitrites or nitrates by means either of ferric oxide, of black oxide of manganese or of gypsum. I can say with Millon,* that, in spite of all that has been written in favor of the oxidation of ammonium by ferric oxide, "I owe it to truth to state that though the most varied attempts have been made to oxidize ammonia in the cold (i. e. in the wet way), by peroxide of iron, they have all proved unavailing."

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* The following statement has a certain interest for analysts as bearing upon the stability of dilute solutions of ammonium chloride. In February, 1877, it was noticed by my assistant, Mr. Lewis, that a solution of ammonium chloride which had been prepared ten or twelve months previously for use in connection with Nessler's test, by dissolving the salt at the rate of 3·15 grams to the liter, now gave a strong reaction for nitrites, although freshly-prepared solutions, made in the same way from like materials gave no reaction. The old solution was contained in a glass-stoppered bottle which was about half filled by it, and it had been kept most of the time in a dark cupboard.

Acting on the supposition that the change of the ammonium salt to a nitrite had been caused by the growth of some fungus in the liquid and that such fungus might perhaps be present in other bottles in the neighborhood, I collected as many different specimens of moulds as could be found growing in the various saline solutions kept in the laboratory, and, after rinsing with pure water, placed them in a series of half-gallon bottles, into which had been poured from half to quarter of a liter of dilute ammonium chloride, such as is used for Nesslerising. A quantity of mother of vinegar also was placed in a similar bottle. This series of bottles was left to stand during early autumn, and winter in a green-house, in a rather strong light, and the solutions were tested from time to time, at intervals of three or four weeks, but no reaction for nitrites or nitrates could ever be obtained from either of the bottles. The experiments of Warington show that these trials were vitiated by the strong light to which the bottles were exposed. It is not altogether unlikely that different results would have been reached if the bottles had been kept in the dark.

I am indebted to my assistant, Mr. D. S. Lewis, for his careful attention to the details of these experiments.

Bussey Institution, Jamaica Plain, Mass., April, 1878.

ART. LXV.—*Geographical and Geological Survey of the Rocky Mountain Region under the direction of Professor J. W. Powell. Account of work performed during the year* 1877.

[Concluded from page 358.]

Ethnographic Work.—During the season the ethnographic work was more thoroughly organized and the aid of a large number of volunteer assistants living throughout the country was secured. Mr. W. H. Dall, of the United States Coast Survey, prepared a paper on the tribes of Alaska, and edited other papers on certain tribes of Oregon and Washington Territory. He also superintended the construction of an ethnographic map to accompany his paper, including on it the latest geographic determination from all available sources. His long residence and extended scientific labors in that region peculiarly fitted him for the task, and he has made a valuable contribution both to ethnology and geography. With the same volume was published a paper on the habits and customs of certain tribes of the State of Oregon and Washington Territory, prepared by the late Mr. George Gibbs, while he was engaged on scientific work in that region for the government. The volume also contains a Niskwalli vocabulary with extended grammatic notes, the last great work of the lamented author. In addition to the map above mentioned and prepared by Mr. Dall, a second was made, embracing the western portion of Washington Territory and the northern part of Oregon. The map includes the latest geographic information, and is colored to show the distribution of Indian tribes, chiefly from notes and maps left by Mr. Gibbs. Much of the linguistic material of this volume was collected by correspondents of the Smithsonian, and turned over to Professor Powell, to be consolidated with materials collected by members of his corps.

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* Chemical News, 1860, ii, 337, from Comptes Rendus.

These papers form a quarto volume of 361 pages, entitled Contributions to North American Ethnology, volume I, the first of a series to be published on this subject.

Volume II. relating to the tribes of the eastern portion of Washington Territory and the State of Oregon, was partially prepared for the printer, but it was thought best to withhold its publication until further materials were collected from that region.

The third volume of the series has been published. This relates to the Indians of California. Mr. Stephen Powers, of Ohio, has been engaged for several years in the preparation of this volume. The first part contains an account of the habits, customs, mythology, etc., of the several tribes. At our earliest knowledge of the Indians of California they were divided into small chieftaincies, speaking diverse languages, and belonging to radically different stocks, and the whole subject was one of great complexity and interest. The materials collected by Mr. Powers were sufficient to successfully unravel the difficult problem relating to the classification and affinities of a very large number of tribes, and his account of their habits and customs is of much interest. A number of vocabularies of the Smithsonian Collection are published with those of Mr. Powers. The linguistic portion of the volume was edited by Professor Powell.

The volume is accompanied by a map of the State of California, compiled from the latest official sources, and colored to show the distribution of linguistic stocks.

The Rev. J. Owen Dorsey, of Maryland, has been engaged for more than a year in the preparation of a grammar and dictionary of the Ponka language. His residence among these Indians as a missionary has furnished him favorable opportunity for the necessary studies, and he has pushed forward the work with zeal and ability.

Professor Otis T. Mason, of Columbia College, has for the past year rendered the office much assistance in the study of the history and statistics of Indian tribes.

Brevet-Lieutenant Colonel Garrick Mallory, United States Army, has during the year been engaged in the study of the history and statistics of the Indians of the United States. His researches lead him to the conclusion that the generally received opinion that a very large Indian population occupied this country at the time of its discovery is erroneous, that the supposed rapid and general decadence of the Indians arising from contact with civilization is not sustained, and that when circumstances have not rendered it impossible, they are making reasonable progress towards civilization, together with which in many instances their numbers have increased. No final publication on the subject has yet been issued, but he has read papers before the Philosophical Society of Washington and other scientific bodies, to invite the attention of ethnologists to the subject. He has also been engaged in preparing the history and bibliography of the Klamath, Chinook, Wayiletpu, Sahaptin, and other families of Oregon, and his papers on this subject will appear in the second volume of Contributions to North American Ethnology.

In March last, Mr. Albert S. Gatschet was employed to assist in the study of Indian languages, and during the spring months his time was occupied as an assistant in compiling the bibliography of the North American languages. During the summer and autumn months he visited a number of tribes in Oregon, for the purpose of collecting vocabularies and grammatic notes. On his way to the field he stopped at Ogden, where he found a tribe of Shoshone Indians, from whom he procured a vocabulary of about five hundred words.

In Chico, Butte County, California, he stopped one week, to visit the Michópdo Indians, a branch of the Maidu stock, where he collected linguistic material of value. From Chico he proceeded directly to the Klamath Agency, in Southern Oregon, where eight weeks were devoted to the study of the language of the Klamath Indians, a branch of the Modok family. Mr. Gatschet had previously studied this language, by obtaining words from Modok Indians visiting Washington and New York, and his work at the Klamath Agency was a continuation of such study. Altogether he has collected a vocabulary of about five thousand words, also many sentences and texts on historic and mythologic subjects arranged with interlinear translations.

The numerical system of this language is quinary, and the numerals above eleven have incorporated particles giving them a gender or classifying significance, apparently based upon form. The subject and object pronouns are not incorporated in the verb; the personal pronouns differ from the possessive; and a true relative pronoun exists. An important characteristic of the language is the use of prefix-particles in nouns and verbs indicating form, and the reduplication of the first syllable, which is usually the radical syllable, for the purpose of showing distribution. It is often equivalent to our plural. It occurs in the singular of adjectives indicating shape and color, in augmentative and diminutive nouns and verbs, in iterative and frequentative verbs; and forms the distributive plural of many substantives, adjectives, numerals, verbs and adverbs.

From the Klamath Agency, Mr. Gatschet proceeded to the Grande Ronde Agency, in the northwestern part of Oregon. On his way he stopped at Dayton, and made collections of

Shasta and Umpqua words, from reliable Indians. On the Grande Ronde Agency are found a large number of tribes and remnants of tribes which were collected there after the Oregon war of 1855–6; and with the exception of the Klikatats they are all from Western Oregon. The following is a classification of the linguistic stocks now on this reservation: Tinnéh, Silets, Wayiletpu, Shasta, T'sinuk, Sahaptin, Selish, Modok and Kalapuya. The Kalapuya once occupied almost the whole extent of the beautiful and fertile Willamette valley, and one branch of this stock, the Yonkálla, even extended into the Umpqua valley.

The Tuálati language, a dialect of the Kalapuya stock, was the one studied by Mr. Gatschet, and from his notes the following characteristics appear: The phonetics are strikingly soft and harmonious, and though consonants are often assembled in large clusters, they never offend the ear, nor do they seem unpronounceable to Americans. A large number of words begin with vowels, especially with *a*, *i* and *u*. The substantive adjective and numerals are not inflected for case, as in the Modok. Adjectives and numerals and some substantives are varied to indicate the plural number. The parts of speech are very imperfectly differentiated. The personal and possessive pronouns have the same gender, as a distinction of sex is indicated in the singular of subject pronouns but not in the plural. Prefix particles are extensively used to express the mood, voice and tense of the verb, and the same particles fulfill this function in the noun. The personal pronouns of the direct object differ greatly from the pronouns of the indirect object; and every one of the three persons, in the singular and plural, possess a different series of direct and indirect objective pronouns. The conjugation of the transitive verb differs in many particulars from that of the intransitive. There appear to be structural affinities between the Kalapuya and Selish stocks. Over three thousand words, many hundred sentences and valuable ethnologic texts were collected.

Besides the Michópdo, Modok and Tuálati before mentioned, Mr. Gatschet also collected vocabularies and sentences of the following languages: Shoshoni, Achomáwi, Shasta, Wintun, Waccanéssisi, Waska, Klákamas, Mólele, Nestucca, Yamhill, Lukamáyuk and Ahantchuyuk. In the collection of all these vocabularies, the "Introduction to the Study of Indian Languages," prepared for the Smithsonian Institution by Prof. J. W. Powell was used.

Dr. H. C. Yarrow, U. S. A., now on duty at the Army Medical Museum in Washington, has been engaged during the past year in the collection of material for a monograph on the customs and rites practised in the disposal of the dead among the

North American Indians. To aid him in this work, circulars of inquiry have been widely distributed among ethnologists and other scholars throughout North America, and much material has been obtained which will greatly supplement his own extended observations and researches.

During the summer some interesting work was done in the examination of the stone graves of Tennessee, and valuable collections were made. Professor Powell has coöperated with the Institution in providing for a more thorough examination of the archæology of the islands off the shore of southern California. This exploration was made by Rev. Stephen Bowers, of Indianapolis, Indiana, and his report will be published with the papers of the survey.

A small volume, entitled "Introduction to the Study of Indian Languages," has been prepared. This book is intended for distribution among collectors. In its preparation, Prof. Powell was assisted by Prof. W. D. Whitney, the distinguished philologist of Yale College, in that part relating to the representation of the sounds of Indian languages. A few preliminary copies have been printed and distributed among gentlemen interested in the study of Indian languages for such addition and emendations as may be suggested preparatory to final publication. A tentative classification of the linguistic families of the Indians of the United States has been prepared. This will be published as soon as the bibliography is ready.

In pursuing these ethnographic investigations it has been the endeavor, as far as possible, to produce results that would be of practical value in the administration of Indian affairs, and for this purpose especial attention has been paid to statistics, the progress made by the Indians towards civilization, and the causes and remedies for the inevitable conflict that arises from the spread of civilization over a region inhabited by savages. It is believed that the labors in this direction will not be void of useful results.

Survey of the Black Hills.—In 1875, a reconnaissance survey was made of the Black Hills of Dakota, by Mr. Walter P. Jenny, with a corps of assistants, under the direction of the honorable Secretary of the Interior. On the return of the party from the field, Mr. Jenny's report relating to the mineral resources of the country was immediately published, but the geographical and geological report was unfinished at that time. This latter work was left in the hands of Mr. Henry A. Newton, his geological assistant, to be completed. On May 28th, 1877, at the request of Mr. Newton, the completion of the work was placed under the direction of this Survey by order of the Secretary of the Interior.

On consultation with Mr. Newton, it appeared wise that he should visit the field again for the purpose of determining certain doubtful points in the geological structure, and to insert on the maps the position of the several towns and roads established in that region since the discovery of gold, and Mr. Newton was employed for this purpose. He had been in the field but a short time when he was prostrated by the sickness which resulted in his death. Previous to his departure he completed his report on the geology of that country, and the map had been placed in the hands of the engraver; the whole embodying all the facts discovered up to that time. Thus, happily, his work will not be lost. It is expected that his report will be published during the present winter, in the shape in which it was left by him.

The death of Mr. Newton makes a serious break in the ranks of the younger and more active geologists of America. He possessed rare abilities, had much experience in field operations, and had received thorough and wise training, and his work in other fields had exhibited his ability. But the great work of his short life will doubtless be his report on the geology of the Black Hills of Dakota.

During the past six years one branch of the work of the survey has been considered of paramount importance, namely, the classification of lands and the subjects connected therewith. The object has been to determine the extent of irrigable lands, timber lands, pasturage lands, coal lands and mineral lands. In general the lands that are cultivable only through irrigation are limited by the supply of water. There are some exceptions to this. Where streams are found in narrow valleys or run in deep cañons, the limit of agricultural land is determined by the extent of the areas to which the water can be conducted with proper engineering skill. In the study of this subject many interesting and important problems have arisen, and many valuable facts have been collected.

From the survey of the timber lands one very important fact appears, that the area where standing timber is actually found is very much smaller than the areas where the conditions of physical geography are such that timber should be found as a spontaneous growth—that is, the *area of timber* is but a small fraction of the *timber region.* The destruction of timber in such regions now found naked, is due to the great fires that so frequently devastate these lands; and the amount of timber taken for economic purposes bears but an exceedingly small ratio to the amount so destroyed. Hence the important problem to be solved is the best method by which these fires can be prevented.

Another subject which has received much attention is the utilization of the pasturage lands; and still another, the best methods of surveying the mineral lands for the purpose of description and identification, that the owners of mines may be relieved of the great burden of litigation to which they are subjected by reason of the inaccurate and expensive methods now in vogue.

ART. LXVI.—*Observations on the Transit of Mercury.* Letter to the Editors from JOHN RODGERS, Rear Admiral U. S. N., Superintendent of the United States Naval Observatory, dated May 11, 1878.

IT may interest your readers to learn that the transit of Mercury, occurring on May 5–6, was very successfully observed at the Naval Observatory, and throughout the country generally. Satisfactory observations of all the contacts were made here, and good observations of the contacts have been reported from the observers of the United States Coast Survey in Washington, and in different parts of the country. Reports of observations of the contacts have been received from the French astronomers at Ogden, Utah, and from the astronomers at Cincinnati, Chicago, Glasgow, Mo., and from amateur observers at New Orleans, Savannah, and various other points; so that we have an abundance of observations of this kind.

Since, however, observations of the contacts give only the correction of the longitude of the planet, the larger part of the appropriation of $1,500 made by Congress for observing this transit was expended in photography. The directors of the observatories at Cambridge and at Ann Arbor, Professors Pickering and Watson, kindly undertook the work of making photographs at their observatories; and a set of the instruments used in photographing the transit of Venus in 1874, was sent to each of these observatories. The method of taking the photographs was the same as that followed in the case of the transit of Venus. The dry-plate process was, however, adopted in the present case, in place of the wet-plate process used in the transit of Venus. The great advantage of the dry-plate process, if it can be used successfully, is evident. The plates were all prepared here by Mr Joseph A. Rogers; and seventy-two plates were sent to each of the observatories, where they were exposed and then returned here for development. The same number of plates were exposed here by Mr. Rogers.

The day at Washington was very favorable, and the plates were exposed here in groups of six, along the chord described by the planet across the disc of the sun. The development shows that all the photographs taken here can be measured accurately; and we think they will furnish data for a very exact determination of the latitude and longitude of the planet on the day of its transit.

The day at Cambridge was not favorable, and the development of the plates exposed there gives very thin photographs; some of which, however, can be measured. At Ann Arbor the day was cloudy during the latter part of the transit, and the plates were all exposed in the earlier part of the day. These photographs are all very dense, but probably all can be measured. Measurements of these photographs will be made as soon as possible and the results published.

A set of the transit of Venus photographic apparatus was furnished the French astronomers, Messrs. André and Angot, at Ogden, for the purpose of comparing the different methods of photographing the transit. These gentlemen report a snow storm during the early part of the day, but clear weather in the afternoon, during which a good number of photographs was secured.

Our experience in photographing transits of planets, and in measuring the photographs, indicates that while the American method is correct in theory, the apparatus needs some change. In order to obtain good measures, the picture should be sharp, and the exposure short. It is probable, therefore, that the reflectors, which now lose about nineteen-twentieths of the light, will have to be changed.

A comparison of the observations of contact with the ephemerides of the American and English Nautical Almanacs shows that the English Almanac is much nearer the truth. Since the ephemeris of the American Almanac is based on Leverrier's old theory, and that of the English Almanac on his recent one, the result of the present observations appears to be a confirmation of Leverrier's theory with respect to an intra-mercurial planet.

United States Naval Observatory, Washington.

ART. LXVII.—*Transit of Mercury of May 6th*, 1878; by S. P. LANGLEY.

THE following observations made during the transit of Mercury, yesterday, have some interest from the inferences to be drawn from them as to physical phenomena; and to devote the opportunity more wholly to this object, no measures of precision were attempted, beyond noting the times of ingress and egress. The principal instruments were the equatoreal refractor of thirteen inches (used in the early part of the day with nine inches effective aperture), and a polarizing solar eye-piece, which dispenses altogether with any dark glass, and presents objects in their natural colors and relative brightness.

I had the fortune, at ingress, of an unusually blue and transparent sky, and aided by this, saw with the polarizing eye-piece the entire disc of Mercury *outside* the sun about one-half a minute before first external contact. Presumably it might have been seen even earlier, had not time been lost in searching for it, through lack of means to designate the precise position-angle, the position filar-micrometer not being adaptable to this eye-piece. After a pause to verify the reality of the phenomenon by revolving the eye-lens, etc., the chronograph key was struck at $21^h\ 52^m\ 39^s{\cdot}45$ Allegheny mean time, to record the observation. As this was really made earlier, and the disc was seen throughout its circumference, it seems clear that the coronal back-ground is bright enough to produce this effect at least fifteen seconds of arc from the solar limb, and in spite of the atmospheric glare.

As a partial substitute for the filar micrometer, there was in the field a glass reticule, ruled (by Prof. Rogers, of Harvard) in squares whose sides represented here $15''{\cdot}3$, and this enabled —not a measurement—but a fair comparison to be made of the apparent size of the planet before and after it entered on the sun. The contrast was striking, as on a back-ground very little brighter than itself its diameter was, if anything, greater than one of the sides of these squares, while as soon as it entered on the sun it seemed to shrink by more than one-fifth of this. First external contact was noted on the chronograph at $21^h\ 52^m\ 50^s{\cdot}43$. First internal contact was noted when the sunlight could be seen unmistakably between the disc and limb at $21^h\ 55^m\ 47^s{\cdot}25$. These entries, I believe to have been made in both cases nearly two seconds late. The limb just at second contact, was steady. I saw no "black drop" or "ligament."

As the disc advanced on the sun it was closely scrutinized, without at any time any "bright point" or "annulus" being seen. These appearances, resting as they do on much testimony, particularly the unimpeachable evidence of Mr. Huggins, I was prepared to expect, but fruitlessly looked for with powers varying from 120 to 800 throughout the day, with the polarizing eye-piece, and also by projection of the image. The phenomenon may depend for its visibility on exceptionally good definition, which Mr. Huggins* appears to have had; that here was fairly, though not unusually, good. The darkest part of the planet was the center, the edges being decidedly less gray. The cause of this gradation came out very clearly in forming a very enlarged image for projection, being plainly due in most part to minute and rapid atmospheric tremor. In moments of best definition the surface became of a nearly uniform shade throughout.†

The planet has been almost uniformly described as looking "black" in transit, but in the instrument I use (the objective of which was corrected by Mr. Alvan Clark), it certainly does *not* look black. The color is decidedly less red than that of spot nuclei, being gray, slightly inclining toward a blue, like that of the spectrum between F. γ. G. (It may be that this bluish cast comes from the secondary spectrum of the objective). The average light from the disc in transit is very considerable, being not much less than that of some nuclei. No spots were present for comparison, but being engaged in photometric determinations of these and other parts of the solar surface, I was provided with means of comparing Mercury with tints which had previously been contrasted with sun-spots under like conditions. Absolute photometric determinations of the apparent light from Mercury in transit were attempted by projecting a greatly enlarged image (its actual diameter was three-quarters inch as projected), on a white surface in a dark camera attached to, and moving with, the equatoreal. Direct measurements with a Jamin photometer were unsatisfactory. Subsequently, by another method, a trustworthy value was fixed for a minimum. It was thus found that the light actually received on the paper apparently from the so-called "black" body of the planet, at any rate exceeded eight per cent of that from direct sunlight, and measures taken by the thermopile and galvanometer showed that heat was coming from the same direction.

It need hardly be said that it is impossible that Mercury itself should be radiating heat and light in any such degree. Accordingly I take these numbers as representing (with some possible allowance for instrumental causes) the minimum effect we can assign to our own atmosphere in inflecting the solar radiation, a subject on which data have been hitherto desirable. It is evident, for instance, that from the facts here stated we can estimate, photometrically, the intrinsic brightness of the corona, since it was undoubtedly this, acting as a back-ground, which enabled the planet, though itself involved to a calculable extent in atmospheric glare, to be seen before it reached the solar limb.

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* Monthly Notices R. A. S., vol. xxix, p. 26.

† I presume that even in absolutely perfect definition there would be theoretically a slight gradation due to another cause, i. e., to the greater effect of the edge of the planet's disc of the inflection, referred to in a subsequent paragraph.

The observations were interrupted by haze in the afternoon and egress was so nearly invisible that the apparent times of contact are not worth giving.

Allegheny Observatory, May 7, 1878.

ART. LXVIII.—*Fossil Mammal from the Jurassic of the Rocky Mountains;* by Professor O. C. MARSH.

ONE of the most interesting discoveries made in the Rocky Mountain region is the right lower jaw of a small mammal recently received at the Yale College Museum. The specimen was found in the Atlantosaurus beds of the Upper Jurassic, and the associated fossils are mainly Dinosaurs.

Dryolestes priscus, gen. et sp. nov.

This specimen is in fair preservation, although most of the teeth have been broken off in removing it from the rock. The penultimate molar, however, remains. The shape of the jaw, and the position and character of the teeth, show that the animal was a small marsupial, allied to the existing Opossums (*Didelphidæ*). The tooth preserved has the same general form as the corresponding molar of *Chironectes variegatus* Illiger. The angle of the jaw is imperfect, but there are indications that it was inflected.

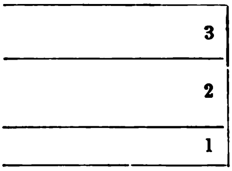
The principal dimensions of this specimen are as follows:

Space occupied by seven posterior teeth	12·5$^{\mathrm{mm}}$
Depth of jaw below last molar	4·4
Transverse diameter	1·8
Height of crown of penultimate molar	2·
Transverse diameter	1·5

The present specimen indicates an animal about as large as a weasel. It is of special interest, as hitherto no Jurassic mammals have been found in this country.

Yale College, New Haven, May 13, 1878.

ART. LXIX.—*On some dark Shale recently discovered below the Devonian Limestones at Independence, Iowa; with notice of the Fossils at present known to be in it;* by S. CALVIN, Professor of Geology, State University of Iowa.



THE Devonian deposits of Iowa, as now known, may be roughly represented by the annexed diagram, in which 1 indicates the position of a member of the group recently discovered at Independence, consisting of dark argillaceous shales with some thin beds of impure, concretionary limestone. It has been explored to a depth of twenty or twenty-five feet. No. 2 represents all of what is usually included under the head of the Devonian limestones of Iowa, and is made up largely of limestone with some associated beds of light-colored shales; estimated thickness, 150 feet. No. 3 is a bed of argillaceous shales exposed at and near Rockford, Iowa, and is referred to frequently as the Rockford Shales. It abounds in fossils, and weathers, on exposure, into a stiff clay that has been utilized in the manufacture of brick; observed thickness, seventy feet.

Until recently, Nos. 2 and 3 of the above section were supposed to make up the entire thickness of Devonian rocks in Iowa. No. 2 not only varies, as already indicated, in lithological characters, but the grouping of fossils differs widely in different localities, so much so that competent geologists have referred certain exposures—for example those at Waterloo—to the Corniferous, and others—as at Independence and Waverly—to the Hamilton. Such reference of the above-named exposures will be found in the Twenty-third Report on the State Cabinet of New York, pp. 223–226, and in the same article Professors Hall and Whitfield declare the Rockford Shales to be the equivalent of the New York Chemung. On the other hand, Dr. C. A. White, Geology of Iowa, 1870, vol. i, p. 187, is of opinion that all the Devonian strata of Iowa belong to a single epoch.

Thus matters stood until about a year or so ago, when D. S. Deering called attention to the interesting fact that a dark shale had been exposed in working out the layers in the bottom of one of the limestone quarries near Independence. The quarrymen penetrated the shale to a considerable depth in the hope of finding coal. The shale varies somewhat, lithologically, but where it presents its most characteristic features, it is argillaceous, fine-grained and charged with bituminous matter. In some of the beds there are numerous remains of plants—stems of Lepidodendrons and Sigillarias that made up the forests of the Devonian. The plants, however, are very imperfect; the form only is partially preserved, and that mainly by pyrite that replaced the original stem. The woody tissue of the plants has been in part converted into coal that occupies thin irregular seams among the laminæ of pyrite. The little bands of coal vary in thickness, but none of them observed exceed a quarter of an inch. None of the plants are perfect enough to render identification or description possible.

The discovery of shale charged with the carbonized stems of plants, below the Devonian limestones of Iowa, is a matter of much interest. Frequent reports have gained circulation of the discovery of coal in drilling wells in regions occupied by Devonian rocks. From Jesup, Janesville, Marion, Davenport and other places, such rumors have gone out. In two or three cases shafts were dug at considerable expense, necessarily ending in disappointment and failure.

The discovery at Independence accounts for these reports. In drilling through the limestones, the lower shale, with its carbonized plants, was reached, and the color of the borings mixed with fragments of real coal naturally enough gave rise to the impression that a veritable coal mine had been found.

It is to be noticed that all the places from which such reports have come stand near the eastern outcrop of the Devonian, where its entire thickness could be pierced at a very moderate depth. The number and position of such localities would show that the shale in question is not a mere local deposit, but is distributed all along the outcrop of Devonian rocks in Iowa.

The researches of Mr. Deering and myself have brought to light quite a number of finely preserved Brachiopods, representing fourteen different species. Of these, two are not determined and five others are new to science, but the chief interest attaches to certain species that have hitherto been known only from the shales of bed No. 3, near Rockford, Iowa. For the purpose of indicating the relationship of the new shale to the other Devonian deposits of Iowa, we shall arrange the specimens in three groups, using my manuscript names for the new species.

I. Species limited in Iowa as far as known, to the Independence Shales: *Strophodonta variabilis*, n. sp., *Gypidula munda*, n. sp., *Orthis infera*, n. sp., *Rhynchonella ambigua*, n. sp., *Spirifera subumbona* Hall.

II. Species ranging through all the Devonian deposits, and so common to beds 1, 2 and 3: *Atrypa reticularis* Lin.

III. Species common to beds 1 and 3, but not known to occur in the intervening limestones: *Strophodonta quadrata*, n. sp., *S. arcuata* Hall, *S. Canace* Hall and Whitfield, *S. reversa* Hall, *Atrypa hystrix* Hall,* and *Productus* (*Productella*) *dissimilis* Hall.

It is an interesting fact that of the twelve determined species, six occur only in the shaly deposits at the opening and close of the Devonian, notwithstanding these deposits are separated by 150 feet of limestones. Only one species is known to pass from the lower shale into the limestone above, and even that appears under a form so altered that specimens from the two beds may be distinguished as readily as if they were distinct species. If we take form and surface markings into account the *Atrypa reticularis* of No. 1, also finds its nearest representative, not in the limestones immediately above, but in the shales at Rockford.

Obviously then the Independence Shales are more nearly related to the Rockford beds than to any other formation in Iowa. The species in group III seem to have disappeared with the ushering in of conditions under which limestones were formed; they maintained themselves in some locality which has not been discovered or from which the shaly deposits have been entirely swept away and returned with the return of conditions more favorable to them during the deposition of the Rockford Shales.

The intimate relation between the two extremes of the group can but strengthen the conclusion of Dr. White, that all the Devonian strata of Iowa belong to a single epoch.

JOSEPH HENRY, LL.D.

PROFESSOR HENRY died, on the thirteenth of May, 1878, at his home in the Smithsonian Institution, Washington City. For over half a century Professor Henry has been one of the foremost men of science in the United States; and his name is well known in all countries where science is cultivated. He was, it is believed, the last of that band of the older men of science in America, dating from the last century, having been the associate during his career, of Hare, Silliman, Bache, Torrey, and others of the same epoch. His eminent attainments and important discoveries early gave him a well earned reputation as an original investigator. Later his skill as an administrator of great public trusts in the interests of science, and his rare personal qualities, made him universally respected and beloved. His noble presence and cultivated bearing were always conspicuous, however distinguished the society about him, and manifested truly the high *morale* which gave him a dignity rarely equalled. His genial smile, not unmingled with a certain reserve, will never be forgotten by those who enjoyed his friendship or came into familiar contact with him. Fortunately, a fine portrait painted quite recently, by order of the trustees of the Smithsonian Institution, will preserve his well known features and render them familiar to posterity.

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* The form designated here as *A. hystrix* Hall, differs conspicuously from the form described in the Geology of Iowa, 1858, vol. i, part 2, p. 515, under the name *A. aspera* var. *occidentalis*. This last form is very abundant in the limestones at Independence. The specimens from the Independence Shale are identical with the form presented by this Atrypa in the Rockford shales. For application of this specific name to this special form, see Twenty-third Annual Report of Board of Regents on New York State Cabinet, p. 225.

Professor Henry was born December 17, 1797, at Albany, New York, where also much of his early life was passed. He had at first the advantages of only a common school education; later, after two years of work as a watchmaker, he came under the training of the Albany Academy, where he developed a degree of mathematical talent which, in 1826, led to his selection for the duties of instructor in mathematics in that institution. Prior to this, having had some experience in the field as a surveyor, he was associated with Amos Eaton in the Geological Survey along the line of the Erie canal, projected and sustained by General Stephen Van Rensselaer.

While occupied with his duties as mathematical instructor in the academy—then in charge of Dr. T. Romeyn Beck—he commenced that line of investigation in electricity which resulted in the important discoveries that have made his name famous. He attended the lectures of Dr. Beck on chemistry, and assisted in the preparation of his experiments. At this time he devised and published an improved form of Wollaston's sliding scale of chemical equivalents, in which hydrogen was adopted as the radix—a contrivance which is hardly known, even by name, to the present generation of chemists. Thus, while Professor Henry's original contributions to science were chiefly physical, his first scientific work was in the department of chemistry. His work with Dr. Beck enabled him, after his removal to Princeton—where he became Professor of Natural Philosophy in 1832,—to take up the duties of the chemist, Dr. John Torrey, when that well known teacher was disabled for a time by ill health.

It was in the interval, between 1828 and 1837, that the most important work of his life was accomplished in the line of strictly scientific research. These results are chiefly recorded in the Transactions of the Albany Institute, the volumes of this Journal for the period, and the Transactions of the American Philosophical Society. His "Contributions to Electricity and Magnetism," were collected in a separate volume in 1839. The analysis of these important researches, and the discussion of the questions of priority connected with them, will be the duty of the academician to whom shall be assigned the preparation of a memoir or eulogy of the distinguished author.

Without assigning dates we give the following brief enumeration of his memoirs and discoveries, taken from a communication made by him to Prof. Silliman in August, 1874, for the purpose of insertion in an address delivered at Northumberland, Pennsylvania, on the occasion of the celebration of the centennial of chemistry. The order and enumeration are Professor Henry's:

1. A sketch of the topography of the State of New York, embodying the results of the survey before mentioned.
2. In connection with Dr. Beck and the Hon. Simeon De Witt, the organization of the meteorological system of the State of New York.
3. The development, for the first time, of magnetic power, sufficient to sustain tons in weight, in soft iron, by a comparatively feeble galvanic current.
4. The first application of electro-magnetism as a power, to produce continued motion in a machine.
5. An exposition of the method by which electro-magnetism might be employed in transmitting power to a distance, and the demonstration of the practicability of an electro-magnetic telegraph, which, without these discoveries, was impossible.
6. The discovery of the induction of an electrical current in a long wire upon itself, or the means of increasing the intensity of a current by the use of a spiral conductor.
7. The method of inducing a current of quantity from one of intensity, and *vice versa.*
8. The discovery of currents of induction of different orders, and of the neutralization of the induction by the interposition of plates of metal.
9. The discovery that the discharge of a Leyden jar consists of a series of oscillations backward and forward until equilibrium is restored.
10. The induction of a current of electricity from lightning at a great distance, and proof that the discharge from a thunder-cloud also consists of a series of oscillations.
11. The oscillating condition of a lightning-rod while transmitting a discharge of electricity from the clouds, causing it, though in perfect connection with the earth, to emit sparks of sufficient intensity to ignite combustible substances.
12. Investigations on molecular attraction, as exhibited in liquids, and in yielding and rigid solids, and an exposition of the theory of soap bubbles. [These originated from his being called upon to investigate the causes of the bursting of the great gun on the United States Steamer Princeton.]

13. Original experiments on and exposition of the principles of acoustics, as applied to churches and other public buildings.

14. Experiments on various instruments to be used as fog signals.

15. A series of experiments on various illuminating materials for light-house use, and the introduction of lard oil for lighting the coasts of the United States. This and the preceding in his office of Chairman of the Committee on Experiments of the Light-House Board.

16. Experiments on heat, in which the radiation from clouds and animals in distant fields was indicated by the thermo-electrical apparatus applied to a reflecting telescope.

17. Observations on the comparative temperature of the sun-spots, and also of different portions of the sun's disk. In these experiments he was assisted by Professor Alexander.

18. Proof that the radiant heat from a feebly luminous flame is also feeble, and that the increase of radiant light, by the introduction of a solid substance into the flame of the compound blowpipe, is accompanied with an equivalent radiation of heat, and also that the increase of light and radiant heat in a flame of hydrogen, by the introduction of a solid substance, is attended with a diminution in the heating power of the flame itself.

19. The reflection of heat from concave mirrors of ice, and its application to the source of the heat derived from the moon.

20. Observations in connection with Professor Alexander, on the red flames on the border of the sun, as observed in the annular eclipse of 1838.

21. Experiments on the phosphorogenic ray of the sun, from which it is shown that this emanation is polarizable and refrangible, according to the same laws which govern light.

22. On the penetration of the more fusible metals into those less readily melted, while in a solid state.

The following paragraph, from a letter of Mr. P. C. VanWyck published recently in the New York Times, has a special interest in this place, although repeating well-known facts, since it comes from one of his pupils, a graduate of Princeton of 1845.

"While attending Professor Henry's lectures the Professor was in the habit of demonstrating to the Senior Class the practicability of transmitting messages over a wire stretched from his lecture-room across the campus to his own study, and receiving answers back from Mrs. Henry—elementary, to be sure, yet nevertheless sufficient to demonstrate the principle. So, also, while lecturing on the subject of 'Sound,' he would have a long pole about an inch in diameter passing from the basement to the philosophical hall in the attic, on which was screwed firmly a rude imitation of a fiddle at the top near the ceiling, while to the other end negro Sam had a real fiddle attached.

On a bell signal from the Professor, Sam would saw away with his bow in the cellar, the Professor calling the attention of the class to the weird music his fiddle discoursed in the lecture-room. On these occasions Professor Henry always remarked that the function of the philosopher ceased when he demonstrated the principles of nature in his discoveries; that it then fell to the share of the inventor, by ingenious devices, to subordinate them to the uses of man."

Dr. Barnard's notice of his life and labors, in Johnson's Cyclopædia, is known to have been revised by Professor Henry. This fact is important in view of the interest attached to the question of priority in the discovery of the principles on which the electro-magnetic telegraph was invented. Henry's discovery in 1830 or 1831 of the production of powerful magnetism, *at a distance*, by the use of an intensity battery current and a reduplication of the windings, in separate short circuits, of conducting wire, and the use of an automatic circuit breaker to sound a bell, anticipates by many years any electro-magnetic telegraph. It is, in fact, in universal use to-day; for all skilled operators read by sound, the record on a paper being practically of little use.

To the above enumerated papers should be added an important series of communications, made chiefly to the National Academy of Sciences during the past four or five years, upon the laws of acoustics as developed in the course of investigations conducted for the Light-House Service in order to determine the various conditions involved in the transmission of fog-signals. These investigations have been carried forward mainly in government vessels, and occupied Professor Henry's close personal attention during many weeks of each season.

Besides these experimental additions to physical science, Professor Henry is the author of thirty reports, between the years 1846 and 1876, giving an exposition of the annual operations of the Smithsonian Institution. He has also published a series of essays on meteorology in the Patent Office Reports, which, along with an exposition of established principles, contain many new suggestions; and, among others, the origin of the development of electricity, as exhibited in the thunder-storm.

Professor Henry remained at Princeton, in the chair of Natural Philosophy until his removal to Washington in 1846, to enter upon the duties of Secretary of the Smithsonian Institution.

Favored by nature with a vigorous constitution, he enjoyed through his long life almost uninterrupted good health. His powerful frame and calm spirit enabled him to sustain with comparative ease the pressure of an uncommon burden of official labor which fell to his share as the head of the Smithsonian Institution; of the Light-House Board; of the National

Academy of Science; and as the adviser of the Government in matters of science.

It was while engaged in discharge of certain experimental work on Staten Island last December, connected with the photometric laboratory of the Light-House Board, that he experienced a partial paralysis, which yielded soon to treatment, but was doubtless the precursor of the nephritic attack to which he succumbed. In April he presided at the opening meeting of the session of the National Academy of Sciences held in the rooms of the Secretary of the Smithsonian, and submitted an address to his associates, read by the Home Secretary, recounting with touching simplicity his recent decline of power, and expressing his desire to be relieved from the cares of the office of President. As a mark of affectionate respect, the Academy unanimously requested him to retain this post during his life—leaving the duties to be discharged by the Vice-President. It was on this occasion that the announcement was made to the Academy, by Professor Henry, and, subsequently, in fuller details, by Professor Fairman Rogers, the Treasurer, of the creation of an endowment to be called "the Joseph Henry fund." This fund consists of forty thousand dollars, securely invested, the income of which is for the support of Professor Henry and that of his family, during the life of the latest survivor. Afterwards the fund is to be transferred, in trust, to the National Academy of Sciences, the income to be forever devoted to scientific research. No more graceful and well-merited tribute of respect and affection was ever bestowed upon a man of science, by the spontaneous offerings of personal friends and associates. Alas! that its honored object should have remained so brief a time to enjoy the peace and satisfaction of this gracious endowment. If it is true that Republics are ungrateful, it is pleasant to know that the absence of imperial and kingly patronage may be compensated by a sovereignty not less potent.

It is a truly fortunate circumstance for the science of this country that a man of Professor Henry's pure and exalted character was so long in a position of such influence in public affairs at the seat of government. By force of his earnest determination that the will of the Testator should be carried out, he has saved the Smithson fund from diversion and absorption in a public library, and from various other schemes, and has succeeded in devoting it "to the increase and diffusion of knowledge among mankind," to cite the words used in Smithson's will. By his prudent management of the finances, this fund, after paying for the building an amount nearly equal to the original fund, and certain losses incident to the ordinary chances of investments, is larger to day by one-half than the original fund paid over to the United States. The policy upon which the Smithsonian Institution was instituted and has been carried forward, was mainly the result of the united efforts of Professor Henry and his friend, and able counsellor, the late Professor Alexander D. Bache, whose skill and success as an administrator stood the test of a long and successful career, at Washington, as the head of the United States Coast Survey, from 1845 to 1863. The history of the successive struggles by which the present organization of the Smithsonian Institution was brought about, will be found in the successive annual reports. That this long period of activity, over thirty years, devoted so largely to work almost purely administrative, was a severe tax upon a man of Professor Henry's great productive power and ability in original research can hardly be questioned. On the other hand, it rarely falls to the lot of any man of science to do so much for the best interests of the entire body of scientific workers, or to succeed so well in securing respect and confidence on the part of the public for scientific results and methods. This is conspicuous in many ways in the history of the Smithsonian Institution, and in nothing more perhaps than in the direction which has been given to the numerous governmental explorations during the past twenty years, under its guidance.

Professor Henry leaves a wife and three unmarried daughters, who have been assiduous helpers in the scientific work of their father, making good to a degree the loss of an only son, whose death in early manhood, was a sad disappointment of parental hopes and youthful promise.

Professor Henry was buried May 16th, in the Rock Creek Cemetery, near Georgetown, D. C. The President of the United States, the cabinet officers, diplomatic corps and members of Congress and of the National Academy, were among the mourners. B. S.

ART. LXXI.—*Letter to the Editors from* DR. B. A. GOULD, *of the Cordoba Observatory, dated Cordoba, March* 20, 1878.

I HAVE before me the first part of three different letters to you, which I have begun during the last eighteen months, but the ceaseless pressure of labor in collecting and arranging materials during this interval has left little opportunity for elaborating, and none for communicating them. At last this phase of the work is drawing to a close, and the objects which I had in view in the beginning may be regarded as essentially fulfilled so far as observations are concerned. The part which remains is but the computation and publication of these results, and is going forward as rapidly as the nature of the case permits.

The climate of Cordoba, which during the first years of my sojourn was peculiarly favorable for astronomical work, has showed itself variable to a remarkable degree. The routine of work with the meridian circle goes forward, however cloudy the sky, provided even a small part of the stars desired can be seen through the haze or between the clouds, and very often the thunder is rolling and lightning flashing over a third part of the sky while observations are making on the meridian. Yet in the last year no observations were possible on more than half the nights in March and April, and only ten nights in these two months could be regarded as clear. In June and July the case was worse yet, for there were but seven nights which were clear, and on twenty-eight not a star could be descried from nightfall till dawn. This year the weather has been equally unfavorable, and during the seventy-nine days already elapsed, there has been but one night on which our observations have not been to some extent prevented or interrupted by clouds. The contrast between such condition of the weather, and that which prevailed some years ago is very striking, and seems to point to some periodic fluctuation.

I am all the more disposed to believe this, since arriving at the results relative to the periodicity of sundry phenomena which came to light in the study of the climate of Buenos Aires, as mentioned in my annual report for the year 1876. The volume containing these results, being the first of the Annals of the Meteorological Office, is already printed—the only one yet published of either the astronomical or meteorological series—and refers to the city of Buenos Aires alone. I hope it may soon reach you; but as the process of distribution is necessarily slow, and attended here with many obstacles, I will mention some of the results to which I allude, and which are there discussed in detail.

In the first place the difficulties which have attended previous attempts to determine the relation between the mean temperature and the number of sun-spots have been found to disappear when the local influences which affect the temperature are removed. I have not at hand the means of knowing what these influences may be in other places, but the thermic wind-rose has sufficed for Buenos Aires. Determining for each year the mean direction of the wind, and deducting its effects from the mean temperature as observed in the same year, there results a curve which follows that of the sun-spot so closely as scarcely to leave anything more to be desired. I have only used the mean of the wind-roses determined for ten days each in spring, summer, autumn and winter; nor has the mean velocity of the wind been brought into the computation. More thorough calculations based on a wind-rose more minutely determined and upon the consideration of the total amount of air of a different temperature flowing in, would doubtless increase the degree of agreement; yet the discordance between the mean temperature observed, and that which results from the empirical calculation does not amount in any one of the twenty years, 1856–1875, to so much as might fairly be attributed to errors of observation.

Thus if we denote by r the relative number corresponding to the mean amount of sun-spots, the normal mean temperature of Buenos Aires in centigrade degrees is 17°·50 —0°·00727r, so that 138 of Wolf's scale correspond approximately to 1° C. in the temperature. Thus after deducting the effect of the wind, I find the mean temperatures, as observed, to have been 16°·47 in 1870, and 17°·51 in 1875, while those which result from the formula are 16°·63 and 17°·49 respectively. The difference 0°·16 for the first named year is by far the greatest which presents itself in the whole series, the next largest one being 0°·10, and the mean discordance only 0°·056. The want of perfection in the wind-rose, and the excessive deflection of the mean direction of the wind in 1870 afford what seems to me a sufficient explanation of the relative largeness of the residual.

It is manifest that if the variations of the terrestrial temperature follow those of the sun-spots, and are thus adequate to account for the correspondence observed between these and the variations of the magnetic declination, all necessity for assuming any direct and transcendental connection between this latter and the disturbance of the solar surface disappears.

It is a source of regret that I have not at disposal here any series of mean annual temperatures observed in Europe or North America, accompanied by the corresponding mean direction of the wind; for I have small doubts that analogous results might be deduced, although it is possible that other local disturbances of the temperature may have to be taken into account beside the effect of the wind. There is one other point in South America for which the investigation may be made, viz: Bahia Blanca, just to the north of the Patagonian coast, and this I intend to investigate as soon as time and opportunity permit.

The mean annual direction of the wind varies through an arc of 60° during the twenty years mentioned, having been 70° in 1857, 77° in 1870, and 130° in 1874 (these angles being counted from the north point through the east). The accordance between its variations and those of the spots, which Mr. Main has found in Oxford, does not hold in Buenos Aires, but the study of the phenomenon has brought to light the existence of two cycles in the numbers which represent the mean annual value of $R \sin \varphi$, or in other words in that component of the mean annual direction which is perpendicular to the meridian. Investigated without any assumption or hypothesis whatsoever, they show two periodic terms of which the lengths are 2·78 and 3·70 years. These periods are respectively the quarter and the third part of 11·11 years, the mean length of the sun-spot period, and their existence opens a field for new inquiry.

Another and yet more marked cycle shows itself in the frequency of the storms, which are so well known and characteristic of the La Plata. Whether we consider the annual number of winds of force five, or six, or seven and upwards, or the mean annual force, the same result manifests itself, viz: a periodic fluctuation, the maximum having been early in 1864, the minimum between 1871½ and 1876, and the duration between eighteen and twenty-three years. The form of the corresponding curve is such that it varies but slightly near the minimum, and the period is so nearly equal to that comprised in the series of observations, that it cannot yet be fixed with accuracy. But I am inclined to believe it will be found not to differ much from twenty-two years, in which event we have still another case of near commensurability with the mean period of the sun-spots.

As regards the astronomical work there is little to communicate as yet. The Uranometry is not yet published, and I am fearful that it was a mistake to undertake the printing in this country; not because it cannot be well and even elegantly accomplished, but on account of the long time which it requires, and the limited amount of type available. I am doing all that seems possible to remedy these difficulties, but it is of course at much expense of time and energy. The Atlas is completed; the numerous technical difficulties in the way of a satisfactory reproduction of the drawings having been obviated by the skill and assiduity of the photolithographer, Mr. Julius Bien of New York. My first assistant, Mr. John M. Thome, to whom more than to any other is due what accuracy the determinations of magnitude may possess, was in the United States during the past year, and personally attended to the proofs. He returned a few weeks since, bringing with him some advanced copies, with which I am quite satisfied.

The Atlas consists of thirteen charts, on the scale of a globe of one meter radius, and contains all stars to the 7·0 magnitude inclusive, within a radius of 100° around the South Pole. To these is added a fourteenth as a sort of a Index-Map, to show the limits of the individual charts, the course of the Milky Way, and the general distribution of the stars. This map of course comprises more than a hemisphere, and is projected on a scale of equal areas; the distances between the parallels of declination decreasing as those between the meridians of right ascension increase. The configurations are necessarily distorted, but the degree of aggregation of the stars is correctly given.

As mentioned in former letters I have ventured upon rather a bold reformation of the boundaries of the constellations, which I earnestly hope may find approval with astronomers generally. Wherever possible, meridians and parallels (equinox of 1875·0) have been employed as boundary lines, and in other cases great circles so far as might well be. Yet the principle has been sedulously followed that no important star, and none habitually designated by a Greek letter, should be transferred to a different constellation. By a slight sacrifice of this principle the symmetry of the adopted boundaries might have been essentially increased, yet I have preferred to err upon what seemed the safe side.

The text will be in English as well as Spanish, but the Latin names of the constellations have been preserved, as the only basis for international accordance in nomenclature.

Twice during the last year the normal clock of the observatory has been stopped by earthquake waves, although the tremors seem to have been felt in neither instance by persons in this vicinity. One of these cases was at the time of the great earthquake which destroyed the town of Iquique, and produced so much destruction along the coast of Peru, Bolivia and Northern Chile. The other was at the time of the severe shock at Mendoza. The moments at Cordoba were of course very accurately given by the clock itself, and accounts carefully obtained from the points of chief disturbance give the same result as on former occasions, viz: that the interval of time between the manifestations at these points and at Cordoba was less than the uncertainty of the watches. This you may remember was the case once before, when the shock was distinctly felt in Cordoba, although the clock was not stopped. The clock of the telegraph-station at Mendoza had been regulated from this observatory only a day or two previous, and the time as shown by the dial agreed with that of my watch in Cordoba. Whether the tremors traveled over the intervening distances or not, it seems at least demonstrated that the interval between their arrival at the two places is less than can be measured without special preparation and precautions.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *The Expansion of the Solid Elements a Function of their Atomic weight.*—Since all gases, under similar physical conditions, contain in equal volumes the same number of molecules, they must all have the same coefficient of expansion, because this expansion represents simply an increase in the energy of the progressive motion of the molecules. For the non-gaseous elements, however, no such simple relation has been hitherto observed. Wiebe has thrown some light upon the subject, by studying the expansion-coefficients of the solid elements in connection with their atomic weights. The quotient of the density of any such element, referred to water as unity, divided by its atomic weight, gives the space occupied by one atom of that element. If the ratio of this value to the coefficient of expansion be obtained, some striking relations appear, as is shown in tabular form for twenty-six solid elements. In the first column the symbols are given, in the second the density, in the third the atomic weight, in the fourth the expansion-coefficient expressed in parts in a hundred million at 40° C., and in the fifth the ratio of this to the quotient of the density divided by the atomic weight. The numbers in the fifth column then, represent the absolute expansion of the atoms, or the coefficients of expansion of the elements referred to their atomic volumes. These atomic coefficients often show simple relations for closely allied elements; thus the value is

the same for iron, cobalt and nickel, the numbers for arsenic, antimony and bismuth are as 1 : 3 : 4, those for zinc and cadmium as 2 : 3, etc. If these coefficients be regarded from the stand point of Lothar Meyer's law of the periodicity of the properties of the elements, and be graphically represented as a function of the atomic weight, they give a curve similar to that of the atomic volume, in which members of natural families have an analogous position. Hence the author concludes that the absolute expansion of the atom is a periodic function of its atomic weight.—*Ber. Berl. Chem. Ges.*, xi, 610, April, 1878. G. F. B.

2. *On the New Metal, Gallium.*—In connection with JUNGFLEISCH, LECOQ DE BOISBAUDRAN, the discoverer of gallium, has worked up the residues obtained from 4,300 kilograms of the Bensberg zinc blende, to obtain more of the new metal. The blende was first pulverized and then roasted, the product treated with sulphuric acid in quantity nearly sufficient to dissolve all the zinc, thus leaving a residue of basic sulphate containing the gallium, the residue dissolved in sulphuric acid, the iron reduced by zinc and the solution precipitated fractionally by sodium carbonate several times, the spectroscope being used to detect the metal. In this way, the gallium became concentrated in a residue weighing 100 kilograms. This was dissolved in sulphuric acid, purified with hydrogen sulphide, and treated with ammonium acetate, the H_2S being continued. The zinc sulphide thus precipitated, carried down the gallium. It was dissolved in sulphuric acid, fractionally precipitated by sodium carbonate, the fractions dissolved in the minimum quantity of sulphuric acid, precipitated by H_2S, diluted with water and boiled. A voluminous basic salt of gallium came down which after purification was dissolved in potassium hydrate, and the metal obtained directly from the solution by electrolysis, using forty Bunsen cells in eight parallel series of five each, the negative electrode having a surface of fifteen square centimeters and the positive of 450. In the cold, long crystalline metallic filaments stood out from the electrode, sometimes three centimeters in length. Above 30°, the metal ran in drops to the bottom of the electrode. In this way sixty-two grams of crude gallium were obtained. Taking the inevitable loss into account, the blende of Bensberg contains $\frac{1}{60000}$ of gallium or sixteen milligrams per kilogram. Purified by straining through a cloth, agitating with hydrochloric acid, and recrystallizing, it was obtained as a hard, slightly malleable metal, which can be extended under the hammer or bent rather abruptly, but is quite brittle. On cooling the fused metal to 10° or 15° below its fusing point, and introducing a platinum wire having a bit of solid gallium on one end, octahedral crystals are obtained, their summits modified by the basal planes. Gallium leaves a bluish gray mark on paper, is permanent in the air, remains brilliant even in boiling water, but tarnishes slightly in aerated water. In fusion it is white like tin or silver, but becomes blue-green on solidifying. Chlorine attacks it readily, evolving much heat and producing a well crystallized,

very fusible and volatile chloride, colorless when pure and deliquescent. Bromine acts less powerfully, giving rise to a white bromide, and iodine a white iodide, both of which resemble the chloride. The atomic weight the authors determined to be 69·9.

DUPRÉ, having received a portion of the metal, has undertaken a study of its salts. Heated to 260° in pure oxygen it undergoes no change. At a dull red, it begins to lose its luster and to be covered with a bluish gray pellicle. At a bright red, this pellicle is thicker, a feeble sublimate being formed. Treated with strong nitric acid at 40° or 50°, it dissolves; and the solution heated to 110°, loses nitric acid. Redissolved in water, evaporated to a syrupy consistence, and placed under a dessicator, the nitrate is obtained as a white very deliquescent mass, which may be dried in a current of hot air at 40°. The dry nitrate heated to 200° loses 63·8 per cent in weight, thus proving it to be a nitrate of the sesquioxide. At a higher temperature the nitrate melts, decomposes, and leaves a white friable mass, gallium sesquioxide. Heated to redness in a current of hydrogen, it partly sublimes and is partly reduced. At a cherry red, the mass is bluish gray, but contains no metal, since it dissolves in nitric and in sulphuric acid with no evolution of gas. Since the latter solution reduces permanganate, it is probably a protoxide salt; a view confirmed by the fact that it does not form an ammonium alum as does the sesquioxide sulphate. At a bright red heat, hydrogen reduces a portion of the oxide to the metallic state.—*C. R.*, lxxxvi, 475, 577, 720, 756, Feb., March, 1878. G. F. B.

3. *On Dimethyl-ethylene, or normal Butylene.*—The amyl alcohol of commerce, as is well known, is a mixture in variable proportions of active or ethyl-methyl-ethyl alcohol $\left.\begin{matrix}C_2H_5\\CH_3\end{matrix}\right\}CH-HH_2OH$ and inactive or isopropyl-ethyl alcohol $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH-CH_2-CH_2OH$. Dehydrated by zinc chloride in the ordinary way, it furnishes an amylene consisting of four different bodies: two soluble in sulphuric acid diluted with half its volume of water, ethyl-methyl-ethylene and trimethyl-ethylene; and two insoluble in this liquid, isopropyl-ethylene and normal amylene. In order to avoid polymerization in the preparation of amylene, ETARD has modified the ordinary process by placing the zinc chloride, about 500 grams, in a spacious metal retort, heating it in a gas furnace to full fusion and running in a thin stream of amyl alcohol, the vapors being condensed in a long worm. The amylene was about one-third of the entire product, boiled between 35° and 38°, was completely absorbed by bromine, and with the exception of 3 or 4 per cent, consisted of isopropyl-ethylene $\left.\begin{matrix}CH_3\\CH_3\end{matrix}\right\}CH-CH{=}CH_2$, produced from the corresponding alcohol. The associated ethyl-methyl-ethyl alcohol as well as the admixed propyl and butyl alcohols came over unchanged. But small quantities of polymers were produced, diamylene only being isolated.

Wishing to try this method with butyl alcohol, in order to obtain normal butylene, **Le Bel** and **Greene** allowed this alcohol to fall drop by drop upon zinc chloride heated to fusion in a mercury bottle, the products being collected first in a washing apparatus consisting of a flask, two Pettenkofer's tubes and a Liebig condenser, all containing diluted sulphuric acid to retain the isodimethyl-ethylene, and then passing through bromine. From a kilogram and a half of butyl alcohol, nearly a kilogram of bromide was obtained, which readily separated into three fractions, boiling at 145°–152°, 152°–154° and 154°–160°, the latter fraction forming three-fifths of the whole, and being $CH_3-CHBr-CHBr-CH_3$, mixed with traces of $CH_2Br-CBr-(CH_3)_2$. No ethyl-vinyl was formed in the reaction. Treated with sodium the reaction was violent, and the products collected in hydriodic acid yielded pure secondary butyl iodide, $CH_3-CHI-CH_2-CH_3$, thus proving the presence of normal butylene or dimethyl-ethylene in the above reaction of zinc chloride on butyl alcohol. There are then two normal butylenes, i. e., those in which the carbon atoms are united to no more than two others; one of these is ethyl-vinyl, $CH_3-CH_2-CH{=}CH_2$ and the other dimethyl-ethylene $CH_3-CH{=}CH-CH_3$. Upon the hypothesis that in uniting with a hydracid, the hydrogen and the haloid play the same part as the hydrogen in the saturated molecule and hence that all the hydrogen atoms attached to the same carbon atom have the same value, both these bodies should yield the same hydriodate, $CH_3-CH_2-CHI-CH_3$; while if the hydrogen of the HI has a different value, the two hydriodic compounds will be isomeric. Preparing carefully the two bodies they were found to be identical in properties, both boiling at 118°–121°, and both yielding a butylene by the action of alcoholic potash which gave a bromide distilling between 153° and 160°.—*C. R.*, lxxxvi, 488, Feb.; *Bull. Soc. Ch.*, II, xxix, 306, April, 1878. G. F. B.

4. *Chemical Composition of Oil of Tansy and Oil of Valerian.*—**Bruylants** has submitted to proximate analysis the oils of tansy and of valerian. The former is a mobile yellow liquid, of sp. gr. 0·923 at 15°, begins to boil at 192° and distils mostly between 194° and 207°, the thermometer rising finally to 270°–280°, leaving a resinous mass about a tenth of the whole. Treated with a concentrated solution of hydro-sodium sulphite and then alcohol, oily drops appear which become crystalline and fall to the bottom. They are pearly plates of tanacetyl-sodium sulphite, soluble in dilute alcohol, insoluble in benzene and ether. Heated with a dilute solution of sodium carbonate, the organic portion is obtained as an oil, of density 0·918 at 4°, remaining liquid at —15° and boiling at 195°–196°. Analysis gave the formula $C_{10}H_{16}O$, to which the vapor density 5·07 corresponds; it is thus an isomer of laurel camphor. By the action of H, the tanacetyl hydride gives an alcohol, $C_{10}H_{18}O$, by abstraction of H_2O, it gives cymene $C_{10}H_{14}$, by action of phosphoric chloride, tanacetene dichloride, tanacetene monochloride and cymene, and with ammo-

nio-silver nitrate it gives the mirror characteristic of an aldehyde. Oxidized with chromic acid it gives acetic and propionic acids, with nitric, it yields camphoric acid. The portion of the oil not acted on by the sulphite proved to be an alcohol $C_{10}H_{18}O$, with a small quantity of a hydrocarbon $C_{10}H_{16}$. Hence tansy oil consists of 70 per cent of an aldehyd $C_{10}H_{16}O$, of 26 per cent of an alcohol $C_{10}H_{18}O$, and of 1 per cent of a terpene $C_{10}H_{16}$. Valerian oil proved to be composed of: (1) a terpene $C_{10}H_{16}$, (2) an alcohol $C_{10}H_{18}O$, (3) borneol formate, acetate and valerianate, $C_{10}H_{17}.CHO_2$, $C_{10}H_{17}C_2H_3O_2$, $C_{10}H_{17}C_5H_8O_2$, and (4) ethylborneol oxide, $C_2H_5.O.C_{10}H_{17}$—*Ber. Berl. Chem. Ges.*, xi, 449, March, 1878. G. F. B.

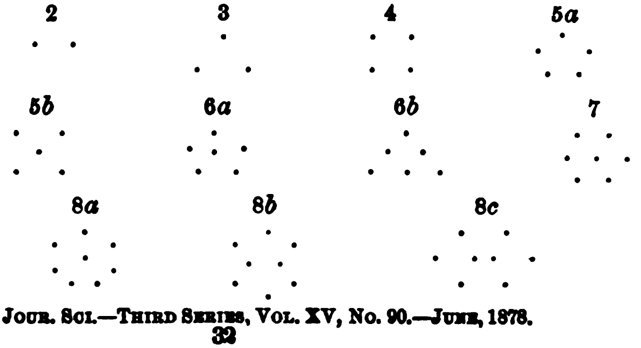
4. *Formation of Xanthin-like bodies from Albuminates in Pancreatic Digestion.*—SALOMON has succeeded by the action of the pancreatic ferment upon pure blood fibrin, in preparing hypoxanthin and very probably also xanthin itself. The former has been observed only as a product of putrefaction hitherto.—*Ber. Berl. Chem. Ges.*, xi, 574, April, 1878. G. F. B.

5. *The present Condition of Electrical Meteorology.*—PALMIERI, the Director of the observatory upon Vesuvius, has published in the Atti della R. Accad. di Napoli, VII, p. 1–20, 1877, a resumé of his observations upon the electricity of the air, which he has conducted during the past twenty-seven years. He also gives a description of the apparatus which he has found best suited to his purpose. The electrometer resembles, at first sight, that of Dellman; it differs, however, essentially from the latter. The suspension of the needle is bifilar and the repulsion between the fixed arm and the needle is not due to the repulsion of two bodies charged by conduction to the same amount, but is the result of induced charges. The needle is provided with a horizontal disc which is carefully centered over a vessel of which it is apparently the cover. This vessel is placed in communication with the testing conductor. The excursions of the point of the needle are read by means of a microscope. The chief peculiarity of the conductor consists in this, that it ends in a plate twenty-seven centimeters in diameter, and the connection with the electrometer is broken just as the conductor reaches the limit of the height to which it is raised, which is about 1·5 meters.

With feeble electrical condition the first swing of the needle is the double of its final deflection. With large differences of potential the fall of deflection is less. This diminution of indication gives a measure of the electrical disturbance. The deflections are compared with those produced by the poles of thirty CuZn elements. A Bohnenberger's electroscope serves as an indicator of the kind of electricity. The director believes that his instrument is preferable to Thomson's self-registering electrometer. The observations tend to show that the electricity of the air is +, and that a negative state of the electricity of the air is due to some local disturbance. The strength of the + electricity of the air increases with the relative moisture of the stratum of air over the apparatus. Simultaneous observations upon the observatory of the University and at the Capodimonte in Naples, also at the observatory upon Vesuvius. More distant observations upon the little St. Bernard and in Moncalieri have impressed the author with the belief that with dry clear air, in which the distribution of + electricity is regular, the strength of the influence diminishes with increasing height.—*Beiblätter Physik und Chemie*, vol. ii, no. 3, p. 155. J. T.

10. *Floating Magnets.*—Nature, for May 2, contains a paper by Sir William Thomson, in which he observes that Professor Mayer's "beautiful experiment [this volume, p. 276], brings us very near an experimental solution of a problem which has for years been before me unsolved,—of vital importance in the theory of vortex atoms;" and also that the experiment had interested him particularly "because the mode of experimenting, with a slight modification, gives a perfect mechanical illustration (easily realized with satisfactory enough approximations) of the kinetic equilibrium of groups of columnar vortices revolving in circles round their common center of gravity, which formed the subject of a communication which I had made to the Royal Society of Edinburgh, on the previous Monday"—that preceding the republication of Professor Mayer's article in Nature—a point which the paper goes on to explain and illustrate.

11. *Note on Floating Magnets;* by Professor A. M. MAYER. (From a letter to the Editors, dated South Orange, New Jersey, May 21, 1878.)—I was much gratified to know that my experiments have proved of so much use to Sir William Thomson. When I received that number of Nature containing Professor Thomson's article on the floating magnets, I was engaged in writing a communication to him, giving him a concise statement of the morphological laws ruling the configuration of the floating magnets, with other new points of interest. These laws I discovered a week or two after I sent you the short note about these experiments, published on page 276. These laws are as follows: the configurations of the floating magnets are divided into primary, secondary, tertiary, quaternary, etc., classes, and the configurations of one class form the *nuclei* to the succeeding ones. The following are the primary configurations:



The configuration found of 9 magnets begins the secondaries; and this configuration has 2 for its nucleus. The secondaries have for nuclei the *stable* primaries, i. e., configurations numbered 2, 3, 4, 5*a*, 6*a*, 7 and 8*a*. The tertiaries have the secondaries for nuclei; the quaternaries, the tertiaries, etc.

If the configuration be made with the superposed magnet at a constant vertical distance, it will be found that when the same number of floating magnets form different configurations, that these different forms always have different *densities*, just as in the phenomena of allotropy, isomerism, etc. In configurations formed of the same number of needles the densities are necessarily inversely as the areas. These experiments beautifully illustrate this, that different crystalline forms, of the same chemical composition, depend on the different *directions* of molecular forces, which in all the forms may have the same intensity.

II. GEOLOGY AND MINERALOGY.

1. *Supplement to the Second Edition of Acadian Geology;* by J. W. DAWSON, LL.D., F.R.S. 102 pp.*—This publication contains the new matter added to the third edition of "Acadian Geology," just issued; and which is published separately in this form for the benefit of those who already possess the second edition. It reviews the new facts which have been discovered in the Maritime Provinces of the Dominion of Canada since 1868. Beginning with the later deposits, the author endeavors to vindicate by new facts his former conclusion that the cold of the Glacial period was not connected with a continental glacier, but with local glaciers on the higher lands and ice-drift by Arctic currents over the plains, then submerged. He subdivides the Post-pliocene deposits as follows, in ascending order:

(*a.*) Peaty terrestrial surface anterior to bowlder clay.
(*b.*) Lower stratified gravels and sands.
(*c.*) Bowlder clay and unstratified sands with bowlders. Fauna, when present, extremely Arctic.
(*d.*) Lower Leda clay, with a limited number of highly Arctic shells, such as are now found only in permanently ice-laden seas.
(*e.*) Upper Leda clay and sand, or Uddevalla beds, holding many sub-Arctic or boreal shells similar to those of the Labrador coast at present.
(*f.*) Saxicava sand and gravel, either non-fossiliferous or with a few littoral shells, of boreal or Acadian types.

After some notice of the Trias, extensively developed in Prince Edward Island, where it has afforded the remains of one Dinosaurian Reptile and several land plants, and which in Western Nova Scotia is so remarkable for its great trappean beds, a large space is devoted to the Carboniferous, and more especially to the recognition of an Upper "Permo-carboniferous" or perhaps truly Permian member, consisting principally of red sandstones, and holding a somewhat peculiar flora akin to that of the Lower Permian of Europe. Details and illustrations are also given of new species of Batrachians, Fishes, Insects, and Crustaceans, recently discovered, and an analysis and comparison with other countries, is made of the remarkable development of the Lower Carboniferous series of Nova Scotia and New Brunswick.

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* New York: Van Nostrand. We are indebted for this notice to Dr. Dawson, the author of the work.

After a short notice of the Devonian, which, in the region referred to, is chiefly remarkable for its rich flora, in the main distinct from that of the Lower Carborniferous, and now numbering 125 described species, the author proceeds to discuss the difficulties attending the study of the Silurian and Cambrian formations, in a region where they are much disturbed and altered, and associated with igneous beds of very varied character. On this subject he remarks:

"In the Acadian Provinces, as in some other parts of Eastern America, the great igneous outbursts, evidenced by the masses and dykes of granite which cut the Lower Devonian rocks, make a strong line of distinction between the later and older Paleozoic. While the Carboniferous series is unaltered, except very locally, and comparatively little disturbed, and confined to the lower levels, the Upper Silurian, and all older series, have been folded and disturbed and profoundly altered, and constitute the hilly and broken parts of the country. Further, in the Upper Silurian and the older periods, there seems to have been a constant mixture with the aqueous sediments in process of deposition of both acidic and basic volcanic matter, in the form of ashes and fragments, as well as probably outflows of trachytic and dioritic rock, so that all these older formations are characterized by the presence of felsite and porphyry and petro-siliceous breccia, and of diorite. Further, since these volcanic and tufaceous rocks, owing to their composition, are much more liable to be rendered crystalline by metamorphism than the ordinary aqueous sediments from which the bases have been leached out by water, and since they are usually not fossiliferous, the appearance is presented of crystalline non-fossiliferous rocks alternating with others holding abundant organic remains, and comparatively unaltered. The volcanic members of these series are also often very irregular in distribution, and there is little to distinguish them from each other, even when their ages may be very different. These circumstances oppose many difficulties to the classification of all the pre-Devonian rocks of Nova Scotia and New Brunswick, difficulties as yet very imperfectly overcome."

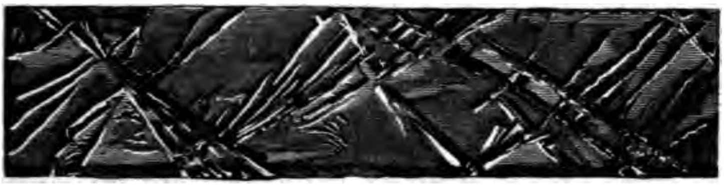
In New Brunswick and in Eastern Maine, it appears that the fossiliferous Upper Silurian rocks are capped by felsites, chloritic schists and agglomerates of great thickness, and having an aspect not unlike that of the older Huronian, while in Eastern Nova Scotia similar rocks appear locally at the base of the Upper Silurian. Again, all the middle part of the Lower Silurian period seems to have been characterized by the deposition of similar volcanic rocks, constituting, with a series of overlying metalliferous slates, the "Cobequid group" of the author, and resembling much more the Skiddaw and Borrowdale formations of the English geologists, than the contemporaneous Lower Silurian groups of inland America. There are, however, undoubted points of resemblance between these peculiar Silurian rocks of the Acadian Provinces and those of New England, and they constitute a remarkable instance of the difference that may obtain in contemporaneous deposits belonging to areas of quiet aqueous sedimentation and of igneous activity. They show very clearly how unsafe it may be, without proper caution, to apply the geological types of one area to those of another.

Below these peculiar Silurian rocks, are thick deposits of Cambrian age, on the whole less modified by contemporaneous igneous action, and in some places richly fossiliferous. In Cape Breton there have recently been recognized fossils indicating an Upper Cambrian horizon, resembling that of the English Lingula Flags. Below this is the Acadian series, so rich in *Conocoryphe*, *Paradoxides* and other forms of the Menevian type, étage C of Barrande. Still lower, according to the author, are the quartzites and slates constituting the gold-bearing series of the Atlantic coast of Nova Scotia. These have recently afforded some peculiar fossils, and appear to be a veritable equivalent of the European Longmynd and Eophyton sandstones. Some portions of this last group, associated with great masses and dykes of intrusive granite, have assumed the condition of gneisses and mica schists, with chiastolite and andalusite. Since the publication of the second edition of Acadian Geology, attempts have been made to relegate them to the Laurentian and Huronian ages, but good reasons are shown for the conclusion that they are merely metamorphosed members of the Lower Cambrian. There are, however, in New Brunswick, and, probably, also in Cape Breton and at the western extremity of Nova Scotia, true Huronian rocks, constituting an upper and lower series, and also true Laurentian, more especially in Southern New Brunswick and Northern Cape Breton. Details on these points, and references to the field geologists who have been working them out, will be found in the publication itself.

2. *Recherches expérimentales sur les cassures qui traversent l'écorce terrestre, particulièrement celles qui sont connues pour les noms de joints et de failles par* M. DAUBRÉE. (Comptes Rendus, lxxxvi, 1878).—M. Daubrée, whose experiments relative to meteorites were referred to in a recent number of this Journal (vol. xiv, p. 510) has completed another series of experiments, having as their object the explanation of the occurrence in rock-masses of joints and faults. The method employed was as follows: a plate of the substance to be examined, in the form of an elongated rectangle, was held at one end in a vice, and by the other extremity was subjected to a torsion or strain about a horizontal axis finally producing fracture. The extreme angle of torsion was 20°.

The best results were obtained when a plate of ice, 80 to 90 cm.

long, 35 to 190 mm. wide, and 7 mm. thick, was employed, it being enveloped in paper so as to preserve the position of the fragments. The most important results were: the formation of fissures whose traces on the outer surfaces of the plate were approximately parallel; there were often two *conjugate* series of these fissures, crossing one another at an angle varying from 90° to 70°, or less, and thus forming a net-like surface; in addition to the fissure-surfaces, there were also a small number of other planes of complete separation or fracture; and finally a series of fine straight lines, like the marks of an engraver, were observed on the surfaces. They were parallel to the fissures and often more regular than they; they indicated the existence of a distinct cleavage whose presence could be proved by a sudden shock. The cut represents a plate which has been subjected to the torsion.



The above results obtained by the torsion or twisting of a plate of ice are shown by M. Daubrée to be closely analogous to the phenomena of faults and joints observed in rock-masses. There is the same approximate parallelism among them; and this is true not only of faults upon a grand scale but also of secondary faults, and minor joints proving them all to have had a similar origin. Moreover it is very common to observe more than one series of these parallel fractures, analogous to the two conjugate series of artificial fissures mentioned above; and though a real difference of age often exists between them it must be in many cases true that they have been produced at the same time. The artificial planes of cleavage described have a parallel in the natural cleavage observed in many rocks, as for example that which gives rise to so-called grain in granite. In short, there appears to be a close similarity between the fractures of different kinds observed in rocks, and those produced artificially in these plates as the result of torsion. From this resemblance in effects M. Daubrée argues an analogy in the causes that have produced them. And he concludes that among the different kinds of mechanical actions, lateral crushings and so on, to which the crust of the earth has been submitted, torsion is one which has played a prominent part in connection with the production of faults and joints.

3. *Notice of a fourth new Phosphate from Fairfield Co., Connecticut;* by George J. Brush and Edward S. Dana.—In our further exploration of the locality mentioned as affording the three new phosphates described in the last number of this Journal, we have found a fourth new species belonging to the same group. The mineral is salmon-colored and proved on examination to be a phosphate analogous to triphylite in composition. It occurs

immediately associated with spodumene and albite and a mineral resembling Shepard's cymatolite. It has generally a bright salmon color and a sub-resinous luster. Hardness =4. Specific gravity =3·424. B.B. fuses at 1 to 1·5 coloring the flame bright lithia red, with streaks of green, and reacts with the fluxes for iron and manganese. Analysis by Horace L. Wells proves it to be a phosphate of manganese and lithia with about four per cent of iron, giving the formula $LiMnPO_4$ or $Li_3PO_4+Mn_3P_2O_8$. We propose to name this new mineral *Lithiolite.* A full description with analyses will be given in an early number of this Journal.

4. *Mineralogische Mittheilungen* (Neue Folge); von G. vom Rath. (From the Zeitschrift für Krystallographie, i, 6, 1877.)—Prof. vom Rath describes:—a remarkable compound crystal of bournonite, consisting of four individuals, but not a true twin; also some new forms upon calcite crystals of Bergen Hill, New Jersey; and crystals of a new mineral, Krennerite. This last mineral, a telluride of gold, was first described by Krenner under the name of "Bunsenin;" as this name has already been used for another species, vom Rath, who describes the crystalline form, proposes the name Krennerite after the discoverer. E. S. D.

5. *Das Erdbeben von Herzogenrath am 24 Juni,* 1877: *Eine seismologische Studie von* Dr. A. von Lasaulx. 77 pp. 8vo. Bonn, 1878, (Emil Strauss). The monograph of Prof. Lasaulx upon the earthquake of October 22, 1873, at Herzogenrath, near Aachen, was noticed in this Journal in Nov., 1874, at p. 392. The earthquake which took place on the 24th of June, 1877, in this locality, had in its phenomena much similarity with the former one, and it has been investigated in the same careful and systematic manner. As the results of the investigation it is concluded that the point from which the shock went forth was at a depth of 16·85 English miles, and that the velocity of propagation was 17·7 miles per minute, the general direction being southwest and northeast. The occurrence of the earthquake is regarded as more or less intimately connected with the great mountain-fissure—the "Feldbiss," which crosses the coal formation of the region of the Wurm in a direction nearly normal to its strike. E. S. D.

6. *Die Mineralogie* von Franz von Kobell. 5th edition, 252 pp. 8vo. Leipzig, 1878. (Friedrich Brandstetter.)—The Mineralogy of von Kobell is now too well known to need commendation here. In the present edition the article upon the chemical constitution of minerals has been altered to some extent with reference to the now accepted chemical principles.

III. Botany and Zoology.

1. *Early Introduction and Spread of the Barberry in Eastern New England.*—On the 10th of June, 1764, the Province of Massachusetts passed "An Act to prevent damage to English grain, arising from Barberry-bushes," with the preamble: "Whereas it has been found, by experience, that the blasting of wheat and other English grain, is often occasioned by barberry-bushes, to

the great loss and damage of the inhabitants of this province; be it therefore enacted," etc.

It is remarkable that this shrub, which is naturalized only along the sea-board of New England, mainly in Massachusetts, and which has up to this time failed to spread anywhere in the interior, should have so multiplied in Massachusetts during the first hundred years as to have made this trouble or obtained this evil reputation. An apparent justification of this ill odor of the Barberry (among the agriculturists we mean) will be found in this Journal, vol. xlix of the second series, 1870, p. 406. Mr. Goodell of Salem, the editor of these old Province Laws, who called our attention to this Act, writes:

"The barberry had, evidently, been widely and abundantly propagated in the older settlements, during the century or century and a quarter of their existence; and it is reasonable to suppose that attempts to exterminate the bushes were not limited to the period of this act's continuance. When the cultivation of wheat, particularly, declined, the farmers, probably, relaxed their efforts against the barberry, and hence may we not account for its present comparative abundance in the vicinity of the older towns, which were near the seaboard? I remember, when a boy, of seeing a single barberry-bush in Athol, Mass., transplanted from somewhere in the eastern counties. It was there considered a remarkable curiosity; but I remember that I wondered why the barberry should not have traveled as far west long before, seeing that the pioneers of that region were, largely, from Essex and eastern Middlesex, and that the bush appeared as thrifty as any I had ever seen in Essex County. The preamble of this act, perhaps, explains the mystery; for there is no reason to doubt that the 'experience' of the farmers therein mentioned was neither recent nor confined to a few, in 1754, when the Legislature, representing a large number of country towns, deemed it necessary to take such vigorous measures for extirpating the barberry."

It has been suggested that the barberry never really damaged the grain-crops of New England, at least to any notable extent; but that the settlers, bringing with them from England the popular fear of it, legislated upon that. But if so, we have a curious illustration of the precarious nature of testimony. For in Europe, this colonial legislation has passed into history as independent evidence that the barberry did damage grain in New England.

A. G.

2. *Ferns of North America;* by Prof. D. C. EATON. Parts IV and V, issued together, bring up the letter-press to p. 113, and the illustrations to plate 15. All but two of these plates carry a couple of species, and the letter-press grows more copious. *Aspidium Nevadense* well fills a plate, and is capitally managed; it is a new species of the Sierra Nevada, the joint discovery of Mrs. Austin and Mrs. Pulsifer Ames, whose names are thoroughly identified with California botany. *Pelicea densa*, of Oregon and California (which does well in cultivation), and *P. pulchella*, a

species which extends from Peru to Western Texas, make a good plate. The supposed raphides of the upper surface of the former may be cystoliths. *Cheilanthas viscida* of Davenport, a new species of Mr. Lemmon's discovery, and *C. Clevelandii* of Eaton, discovered by D. Cleveland of San Diego, form the best plate of this issue, except perhaps that of *Aspidium unitum*, var. *glabrum*, which turns up in Florida. *Anemia Mexicana* and *A. adiantifolia* are well given. *Asplenium Ruta-muraria* is fairly well done, and *A. septentrionale* makes little show at best; and what serves for analysis of both is wretched. A. G.

4. *Dictionnaire de Botanique;* par M. H. BAILLON. Paris. (Hachett & Co.).—This work has proceeded to the eighth fascicle, to p. 640, and to near the end of Ca. The affluence of illustration continues. A. G.

5. VARGAS *considerado como Botanico*, etc., por A. ERNST. Carăcas, Dec., 1877. 4to.—This is a memorial discourse upon Dr. Vargas, pronounced before the Venezuela Society of the Physical and Natural Sciences, upon the occasion of the translation of his remains to the National Pantheon. To this is added the correspondence of Vargas with De Candolle and his friend Mercier; a list of the plants mentioned in the Prodromus as coming from Vargas; and, finally, the Candollean genus dedicated to Vargas having been suppressed by the present writer, a new genus, *Vargasia*, of *Ternstrœmiaceæ*, near *Marcgraria* and *Ruyschia*, is proposed and characterized by Dr. Ernst, and two species described. A. G.

DR. THOMAS THOMSON, the school-mate and associate, in travel and publication on Indian Botany, of Sir Joseph Hooker, son of the distinguished chemist and professor at Glasgow half a century ago, died at London, April 18th, after a long illness. The Gardener's Chronicle of April 27, gives an appreciative biographical notice. A. G.

IV. ASTRONOMY.

1. *Transit of Mercury.*—The transit of Mercury was observed at New Haven on the 6th of May by Messrs. J. J. Skinner, W. F. Beebe and H. A. Hazen. The following are the results of the observations in Washington mean time; the phases being those described in the Washington instructions. Clouds prevented observations at some of the contacts.

	External Contact.	Phase I.	Phase II.	Phase III.	Diam. glass.	Power.	Obs.	Location.
	h m s	h m s	h m s	h m s				
Ingress	22 4 43·	22 7 17·	22 7 32·	22 7 42·	4·6 in.	90	S.	N. Sh. Hall.
Egress	5 36 54·	5 33 58·	5 33 48·	5 33 18·				
Ingress	....	22 7 19·2	22 7 32·1	22 7 49·4	5· in.	180	B.	Athen. Obs.
Egress	5 37 2·2	....	....	5 32 44·2				
Ingress	....	....	....	....	9·6 in.	210	H.	S. S. S. Obs.
Egress	5 36 51·1	5 33 57·8	5 33 48·8	5 33 30·8				

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The International Geological Congress.*—The time for the opening of this Congress in Paris is now finally fixed, by the local committee, for the 29th of August, and the Congress will remain in session about a fortnight. Further details as to organization and place of meeting will soon be made public. Meanwhile, it is announced that from the 20th of August to the 15th of September, the library and reading-rooms of the Geological Society of France, No. 7 rue des Grands-Augustins, Paris, will be at the service of members of the Congress. As before, it is requested that all those who desire to take part therein will make it known to the General Secretary, Dr. Ed. Jannetaz, at the above address, where, also, the subscription of twelve francs, required for each member, may be sent to Dr. Bioche, treasurer. Ladies are admitted to the Congress.

The local committee add to the above announcement as follows: There is reason to believe that the numerous collections of geology and paleontology (minerals, rocks, fossils, maps, plans, sections, models in relief, etc.) to be found in the *Exposition Universelle*, will realize the expectations, expressed in the circular of the International Committee, of an International Geological Exhibition. All exhibitors of such collections are requested to send, as above, such lists as will enable the secretary-general, Dr. Jannetaz, to prepare a special catalogue of them for the use of the Congress.

T. STERRY HUNT, *Secretary of the International Committee.*

2. *Session of the National Academy of Sciences in April.*—List of papers read before the National Academy of Sciences, at the April session, 1878:

Formation and structure of Alacrane Reef on the Yucatan Bank; by A. AGASSIZ.

The theory of waterspouts; by WILLIAM FERREL.

Report on the orbits of the satellites of Mars; by ASAPH HALL.

On the relation of lœss and drift to secular disintegration; by R. PUMPELLY.

On the characteristic invertebrate forms of the central zoo-geographical province of the United States; by A. S. PACKARD, Jr.

On an optical ocean-salinometer; by J. E. HILGARD.

Preliminary report on the deep sea dredgings of the U. S. Coast Survey steamer "Blake" during the past winter in the Gulf Stream and the Gulf of Mexico; by ALEXANDER AGASSIZ.

Abrasions on the northwest coast of America; by GEORGE DAVIDSON.

On the law of Boyle and Marriotte; by WOLCOTT GIBBS.

Abstract of a memoir on the intersection of circles and the intersection of spheres; by BENJAMIN ALVORD.

Biographical memoir of Louis Agassiz; second part. Relating to his life and work in America; by ARNOLD GUYOT.

Biographical memoir of Jeffries Wyman; by A. S. PACKARD, Jr.

Plan for measuring the velocity of light; by SIMON NEWCOMB.

On the force of effective molecular action; by WILLIAM A. NORTON.

Remarks on the value of the result obtained for the solar parallax from the English telescopic observations; by C. H. F. PETERS.

On the vertebrate fauna of the Permian period of the United States; by E. D. COPE.

Report of progress on the subject of "oxygen in the sun;" by HENRY DRAPER.

Photometric comparisons of the components of close double stars; by E. C. PICKERING.

On the duplication of geographical names; by F. V. HAYDEN.

Characteristics of some of the lower spectral lines; by S. P. Langley.

A new element of the cerium group; by J. Lawrence Smith.

On the primary zoo-geographical divisions of the globe and their relations; by Theodore Gill.

Mr. Wallace and Mr. Allen on geographical distribution, with special reference to the alleged distinctness of the Nearctic region; by Elliott Coues.

On the structure and origin of mountains, with special reference to recent objections to the contractional theory; by Joseph LeConte.

Photometric measures of certain faint stars and satellites; by E. C. Pickering.

Contributions to meteorology (ninth paper); by Elias Loomis.

Recent displacements in Utah; by G. K. Gilbert.

On the laws governing the movements of the Rocky Mountain locusts; by C. V. Riley.

Supplementary notice on the paper, "Whence came the inner satellite of Mars," read at the October session, 1877; by Stephen Alexander.

3. *Bulletin of the United States National Museum.* Department of the Interior. No. 10. Contributions to North American Ichthyology, by D. S. Gordan. No. 2. 120 pp. 8vo, with 45 plates.

4. *Payen's Manual of Industrial Chemistry*, edited by B. H. Paul. 987 pp. 8vo, with 698 figures in wood. New York, 1878. (John Wiley & Son).—This translation of Payen's well-known "Précis de Chemie Industrielle," is an acceptable addition to our chemical literature. We learn from the title page that it is "based upon a translation (partly by T. D. Barry) of Stohman and Eugler's German edition" of Payen. The work of the translator is well done and the whole work, as a systematic treatise upon industrial chemistry, has a high value. To those who possess few books in chemistry the addition by the editor of several chapters on general chemistry will be found valuable. The volume extends to 987 pages octavo, and the text is in fine type—too fine to be convenient for many readers—so that the amount of matter contained in the work is very large. There are omissions, as, for example, the facts relating to metal plating; those connected with the zinc process for silver production; and some of the recent improvements in the sulphuric acid processes. Being a French work, the neglect to mention American methods in technology is not a surprise. The work is still indispensable to all interested in the industrial chemistry.

5. *Smithsonian Institution.* — The office of Secretary of the Smithsonian Institution, left vacant by the decease of Professor Henry, has been filled by the appointment of Professor S. F. Baird, who has for a long time held the position of Assistant Secretary, and is eminently fitted for the higher place by his long and active experience in the affairs of the Institution.

6. *American Association.*—The American Association for the Advancement of Science will hold its next meeting in St. Louis, commencing with the third Wednesday in August. Professor Marsh is the President of the Association for the meeting and the ensuing year.

7. *British Association.*—The next meeting of the British Association will be held in Dublin. It will open on the 14th of August.

8. *Rate of Earthquake Wave Transit.*—Under this title, in the Philosophical Magazine for May, Mr. Mallet has a reply to General H. L. Abbot's paper on page 178 of this volume.

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